RESEARCHES,

PHYSIOLOGICAL AND ANATOMICAL.

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BY

JOHN DAVY, M.D. F.R.S.

ASSISTANT INSPECTOR OF ARMY HOSPITALS.

IN TWO VOLUMES.

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TO

THE DIRECTOR GENERAL

OF

THE ARMY-MEDICAL DEPARTMENT,

AND TO THE

MEDICAL OFFICERS OF THE ARMY,

THESE VOLUMES

ARE RESPECTFULLY INSCRIBED.

"Qui de Natura, tanquam de re explorata, pronunciare ausi sunt, sive hoc ex animi fiducia fecerint, sive ambitiose, et more professorio; maximis illi philosophiam et scientias detrimentis affecere. Ut enim ad fidem faciendam validi, ita etiam ad inquisitionem extinguendam et abrumpendam efficaces fuerunt."

BACON, *Præf. ad Novum Organum.*

ADVERTISEMENT.

A CONSIDERABLE portion of this Work has already appeared in print. I have recourse to republication, partly for the purpose of bringing together papers which are scattered in the Transactions of Societies, and in Journals not easy of access; and partly with a view to alterations, corrections, and additions, which either after-reading, or further research have enabled me to make; and lastly, with the hope that the collection thus formed may prove of use, both to the medical student and the medical inquirer.

J. D.

FORT PITT, CHATHAM,
September 7th, 1839.

CONTENTS OF VOL. I.

EXPLANATION OF PLATES.

VOL. I.

For all the Figures in the Plates belonging to both volumes, I am indebted to Mr. Ford, by whom they were drawn on stone, either from the objects themselves preserved in spirits, or from drawings (with one exception) carefully made of the objects when fresh.

PLATE I.

Fig. 1.—Brain of Torpedo; its under surface, shewing the origin of the cerebral nerves.

a the olfactory.
b optic.
c motor.
d pathetic.
e 5th.
f, *g*, *h* } electrical.

Fig. 1. B.—Brain of Torpedo, in situ, shewing its upper surface; the anterior cavity of the cranium, which increases with the size of the fish, and contains a transparent fluid; and the cells of the internal ear (*i*) : the other references are the same as in the preceding.

Fig. 2.—Spinal cord of Torpedo, its upper portion, shewing the origin of the fasciculus of nerves, referred to page 38.

Fig. 3.—Portion of aorta of Torpedo, shewing in two of its branches a bulb-like enlargement, described page 43, probably contractile and muscular, auxiliary in function to the heart.

PLATE II.

Fig. 1.—The female generative organs of the torpedo (T. oculata).

a a the ovaries; the ova in each in a different stage of developement.

b infundibulum of oviduct.

c c uterine cavities; one laid open.

d d the openings of the uterine cavities into the cloaca.

Fig. 2.—Uterine cavity of Torpedo diversicolor, shewing its internal structure, its broad lamellar villi, and the rudimentary gland (*a*).

Fig. 3.—Uterine cavity of Torpedo oculata, shewing its filamentous villi and rudimentary gland (*a*).

PLATE III.

Fig. 1, 2, 3.—Embryo of Torpedo attached to egg in different stages of progress, referred to pages 57 and 58.

Fig. 4, 5.—The same still more advanced, referred to page 59.

PLATE IV.

Fig 1.—Fœtus of Torpedo, referred to page 59; the cavity of the abdomen laid open, exposing the stomach, portion of intestine, and internal yolk.

a external yolk in progress of diminution.

b internal yolk in progress of increase.

c stomach.

d portion of intestine.

e e electrical organs.

f f vestiges of branchial filaments.

Fig. 2.—Fœtus of the same more advanced; the substance of the egg diminishing externally and increasing in the internal yolk vesicle. The probe marks the communication between the internal yolk vesicle, and the upper

part of the small intestine, by the vitello-intestinal canal. The letters of reference are the same as in the preceding, *f f*, only being omitted, the branchial filaments having entirely disappeared.

PLATE V.

Fig. 1.—A male fœtal Torpedo, its internal yolk greatly diminished; the cavity of abdomen distended, from accumulation of the substance of the egg internally.

Fig. 2.—A young Torpedo, six weeks old; the cavity of the abdomen laid open.

a the internal yolk greatly diminished in bulk.
b its connection with the umbilicus, almost absorbed; the external yolk having entirely disappeared.
c the stomach much increased in size.
d portion of intestine.
e e the two lobes of the liver.

PLATE VI.

Fig. 1.—A male Torpedo six months old; the cavity of the abdomen laid open, referred to page 60, shewing a vestige only of the internal yolk vesicle and of the canal, by which it communicated with the external.

Fig. 2.—A fœtal Torpedo, one of ten extracted alive from the uterus, referred to page 72; without external yolk, the abdomen distended, by the substance of the egg accumulated internally.

PLATE VII.

Fig. 1.—A fœtal Torpedo, the same as Fig. 2. of preceding Plate, the cavity of the abdomen laid open, to show the state of the internal yolk vesicle.

a the vesicle of great size.
b portion of intestine.

Fig. 2.—A fœtal torpedo, with a fasciculus of filaments (*a*) attached to the head (similar to the branchial filaments), referred to page 67.

PLATE VIII.

Skeleton of Torpedo oculata, viewed from above.

PLATE IX.

Fig. 1.—Skeleton of the same fish, inverted.

Fig. 2.—The anal appendages of the same, characteristic of the male.

PLATE X.

A young Torpedo: the integuments of its upper surface in part removed, and in part reflected back, and one of the electrical organs detached, to show one of the clusters of mucous glands and a portion of the system of mucous tubes.

a a cluster of mucous glands, from whence tubes for conveying the secretion proceed to the skin.
b an electrical organ exposed.
c electrical nerves.

PLATE XI.

A young Torpedo oculata, showing, without dissection, the distribution of its mucous tubes, connected with the integuments of the back.

PLATE XII.

Fig. 1.—The stomach of the Torpedo, showing its form and that of some of the parts with which it is connected.

a junction of œsophagus and stomach.
b pyloric portion of stomach.
c valvular intestine.
d spleen.
p pancreas.

Fig. 2.—A young Torpedo oculata, with hair or bristle-like filaments growing from its tail and the margin of its temporal apertures, (*a a*).

PLATE XIII.

Fig. 1.—The gills of the Tunny reduced; *a a*, the branchial ganglionic nerves, of large size, referred to page 220.

Fig. 2. A. B.—The basilar artery, exhibiting varieties of the peculiarity of structure, described page 301.

Fig. 3.—A band at the entrance of one of the vertebral arteries, with a filament proceeding from it.

Fig. 4.—The basilar and vertebral arteries laid open, shewing a communication between the latter, close to their termination; a probe indicates the foramen.

Fig. 5.—Microscopal appearance of the fluid of the vesiculæ seminales, referred to page 339; magnified about 300 diameters.

Fig. 6.—The carotid artery of a dog, in progress of healing, after having been partially divided; *a*, as seen externally; *b*, internally.

Fig. 7.—The carotid artery, similarly wounded: the healing process more advanced; *a* and *b* refer to the outer and inner surfaces.

Fig. 8.—The carotid artery, which had been similarly divided; the wound completely closed at the end of six weeks: not a trace of the incision (after immersion in spirits) remaining.

ERRATA.

Page 31,	line	1,	*for* bitmus, *read* litmus.
39,	note,		*for* ii. and iii. *read* x. and xi.
89,	line	14,	*after* long, *insert* reduced.
173,	,,	24,	*for* accompany, *read* accompanies.
219,	note,		*for* p. 218, *read* 191.
220,	line	23,	*omit* natural size.
228,	,,	17,	*for* 28, *read* 1828.
230,	,,	16,	*for* branchia, *read* bronchia.
256,	,,	18,	*for* hours, *read* days.
274,	,,	9,	*for* right, *read* left.

PHYSIOLOGICAL AND ANATOMICAL RESEARCHES.

I.

AN ACCOUNT OF SOME EXPERIMENTS AND OBSERVATIONS ON THE TORPEDO.

THE extraordinary property which the Torpedo possesses of imparting shocks similar, as far as sensation is concerned, to those of the Leyden vial, was one of the last subjects to which the attention of my brother, the late Sir Humphry Davy was given. In the memoir which I have published of his life, particulars are related, shewing how great was the interest he took in the inquiry, and how anxious he was that it should be prosecuted,—and how he urged me to enter on it. The last letter he ever addressed to me, written from dictation when he considered himself dying, is strongly expressive of his feelings. I shall insert it,—partly as an expression of the feelings, and partly as shewing what he considered one of the chief desiderata in the inquiry.

Rome, Feb. 25th, 1829.

"MY DEAR JOHN,

"If I had not had this attack, it was my intention to have gone to Fumicina, or Civita Vecchia, to make some experiments on the torpedo. I hope you will take up this subject, which, both as a comparative anatomist and chemist, you are very capable to elucidate. You will see my paper on the torpedo, in the manuscript book which I have left in Mr. Tobin's hands.* It was my wish to have exposed an unmagnetised needle to the continued shocks of a torpedo in a metallic spiral, making the metallic communication perfect with both electrical organs. There is in my little box, an apparatus which I hope you will use. Large living torpedos may be procured at Fumicina, or Civita Vecchia. The shock from a very small jar will make a needle magnetic, provided it is entirely passed through the metallic conductors; but I did not find this effect when there was any interruption by water. There are many things worth attending to in the two kinds of torpedinal fishes found here—the tremula and occhiatella. Pray do not neglect this subject, which I leave to you as another legacy. God bless you, my dear brother, your affectionate Friend,

"H. DAVY."

* This, his last paper and communication to the Royal Society, was published in the Philosophical Transactions, for 1829; it was written in the preceding October; its results were entirely negative.

The researches which I instituted in compliance with these his wishes, I had the satisfaction of commencing at Rome, in his presence, and of thinking that they afforded him amusement in his last days; they were continued in Malta, and finished in England. The experiments on the living fish were entirely made in Malta, under very favourable circumstances: the anatomical part of the enquiry was commenced at Rome. Most of the results have already been published in the Philosophical Transactions, in two papers, — the first of which was read at the Royal Society in March, 1832, and the second in June, 1834. I may premise that when I entered on the enquiry, it had not been ascertained, that the electricity of the torpedo, considering its peculiar influence to be electrical, had either the power of acting electro-chemically, in separating the elements of any compound bodies; or magnetically, either in affecting the needle in the multiplier, or in imparting magnetism to iron; or lastly, of generating or producing heat: points to which my experiments were particularly directed, and with positive and successful results. I may further remark, that in describing the results obtained, I shall not strictly follow the same order as was observed in my first publication, or restrict myself entirely to the details given in them: what in those were necessarily disjointed, will here be incorporated; and I shall make without hesitation

such other alterations as any additional knowledge I have since been able to attain by my own labours, may seem to render advisable.

1.—*Experiments on the Electricity of the Torpedo.*

From the preceding letter, it appears how desirous my brother was of trying the effect of the shock of the torpedo on a needle placed in a spiral wire. The result, he was of opinion, would be conclusive as to the nature of its electricity,—that is, whether it should be considered distinct, and of a peculiar kind, or merely a variety of common electricity, or at least analogous to kinds already known.

Anxious to make this trial, I had an apparatus in readiness, which, with common electricity, I had found to answer extremely well. It consisted of a fine copper spiral wire, (the spring of a bracer) about one inch and a half long, and one-tenth of an inch in diameter, containing about one hundred and eighty convolutions, and weighing about four grains and a half. This was inserted into a glass tube, just large enough to receive it, and secured by corks.* The wire passed through the cork at each end, and was connected with strong wires with glass handles for the purpose of contact. The wire in-

* The apparatus referred to by my brother, in his letter, did not answer so well: the spiral was of silver wire wound round a glass tube.

tended to be applied to the under surface of the fish was one-twenty-fifth of an inch in diameter; that intended for the upper surface was stiffer, being one-fourteenth of an inch in diameter, and its greater strength was useful, as it was necessary to employ it occasionally with some force to rouse the fish when averse to give a shock. The first trial I had an opportunity of making with this apparatus, was successful. It was on the 3rd of Sept. 1831, at eleven o'clock at night.* The fish used was a small one, about six inches long; it had been just caught in a hand-net, and immediately put into salt water, and was very active. A needle perfectly free from magnetism, was introduced into the spiral, and there confined by the corks, and the spiral was carefully connected with the insulated wires for contact. The fish for the experiment was placed in a glass basin, and was barely covered with water. One wire was applied to the under surface of the electrical organ, and the other to its upper surface, and contacts were made at intervals during about five minutes, when the fish seemed much exhausted by its exertions. On taking the needle out, and bringing it near some fine iron filings, it was found to be magnetic, and

* It may appear singular that this experiment was not made before. The explanation is easily made.—On my return from Rome to Malta, in June, 1829, I was assured that the torpedo is not known in the latter place. It was not until the summer of 1831 that I found out that I had been misinformed, and that, with a little trouble, the fish may be procured alive at all seasons of the year.

powerfully attracted them. This experiment I have repeated many times with fishes of different sizes, some larger and some smaller, and with the same result whenever the fish has been active, and the contacts similarly made.

The next trial instituted on the electricity of the torpedo was on the multiplier; it was one which belonged to my brother, the needle was poised on a pivot, and was not very sensible. The precaution was taken to insulate the instrument well, by smearing with sealing wax the feet of the stand supporting the coil. The same wires for contact were used in this as in the former experiments, and the junctions were carefully made. Applying one wire to the under surface, and the other to the upper surface, using the fish first mentioned, and after an interval of only two hours, I succeeded in obtaining decisive results; the first shock had a powerful effect, the needle made half a revolution: and other trials were in accordance. The needle, by active fishes, was generally thrown into violent motion, occasionally describing nearly a circle, and even by the feeblest it was distinctly affected. I have met with no instance of a fish which had the power of magnetising a needle in the spiral wire, failing to move the needle in the multiplier; but I have met with more than one example of a fish, whose electricity was equal to the latter effect, and not to the former.

I have not mentioned, in my paper in the Philo-

sophical Transactions, that I made comparative trials with the same instruments on the salt water alone by contact. Referring to my notes, I find this distinctly mentioned, and that no effects were perceptible; neither the needle in the spiral acquired the slightest degree of magnetic power, nor did the needle of the multiplier experience any divergence: fully satisfying me that the effects were owing to the peculiar power of the fish. Indeed, that this power was the cause, was manifest from the variability of the effects, in accordance in degree of strength with the energy of the torpedo, as indicated electrically by the sensation which its shock imparted.

The experiments which I have instituted, with a view to ascertain if the electricity of the torpedo has any igniting power, or power of passing through air, and of producing light, have been attended with less satisfactory results. Very active fishes were tried on circles of perfect conductors, interrupted only by a space just visible with the aid of a powerful magnifier. The terminal wires, coated with sealing-wax, excepting at their extremities, were introduced through a perforated glass stopple, into a small glass globe, which was held in the hand of an assistant. The contacts were made in the dark; but not the faintest spark could be perceived, nor could any ignition be perceived when the extreme points were connected with silver wire, not exceeding one-thousandth of an inch in diameter.

When a torpedo was put into a metallic vessel,

insulated by a glass stand, and contacts were made on its back with the insulated wire resting on the edge of the vessel, or at a distance from it, luminous appearances were frequently produced, sometimes in the form of sparks, and sometimes in the form of flashes, not unlike summer lightning on an infinitely minute scale. At first, I was disposed to consider the phenomena electrical; but, on reflection, it occurred that they might depend on the presence of animalcules, which became luminous when agitated. And this I believe is the correct explanation of the effects; for when the salt water was agitated without the torpedo, sparks of light now and then were seen, and the flashes or coruscations might have been owing either to luminous matter thrown off from the surface of the fish, when it gave a shock, or to the shock simultaneously stimulating several particles, which in consequence shone for an instant.

The only positive result which I have obtained on the passage of the electricity of the torpedo through air, has been by using a chain as a substitute for a wire of communication. It was a small gold chain, composed of sixty double links, each circular, and about one-tenth of an inch in diameter, fastened, unstretched, to a dry glass rod at each end. Holding the upper portion of this chain in one hand, and the under wire in the other (the hands being moistened), I irritated by means of them the upper and under surface of an active fish; the shock which it gave was pretty strong, reaching beyond the fingers,

and was felt with the same degree of force in both hands. This seems to shew that the air is not impermeable to the electricity of the torpedo; and the same conclusion may perhaps be drawn from the facility with which I have found it to pass through a circuit of wire in which there have been no less than seven joinings, and those made merely with ordinary care with the fingers, without the aid of any instrument.

In accordance with Mr. Walsh and my brother, I have in no instance seen the torpedo affect the electrometer, or exhibit any the slightest indication of a power of attraction and repulsion in air.

It having been stated on high authority that a spark has been obtained from the gymnotus electricus,* I thought it right, after obtaining the preceding results which were detailed in my first paper,—to renew the attempts to procure a spark from the torpedo. I have tried the method, which, it is said, succeeded with Mr. Walsh in the instance of the gymnotus, namely, dividing with a penknife gold leaf attached to glass, and connecting the divided parts with the contact wires. Using an active fish, in this way, I could neither observe a

* Mr. Walsh is said to have written to M. Le Roi to the above effect; and also that Sir John Pringle and M. Magellan assured M. Le Roi they had witnessed the result repeatedly. Vide Bloch's Ichthyologie, p. 1020. Bloch refers for his information to Rozier's Journal, Ann. 1774. M. De Humboldt (Annales de Chimie et de Physique, tom. xi. 427,) states that the same result has been observed by M. Fahlberg. He refers to Vetensk Acad. ny. quart. 2. (1801).

spark in the dark,—nor in the light detect the slightest indications of the passage of electricity, either by the galvanometer, or the test of the sensation or shock. I have been equally unsuccessful in using an electroscope, formed on the principle of Coulomb's, which displayed sparks when touched either by a small rod of glass slightly excited, or of sealing-wax;—even when the torpedo was taken out of water, and all loosely adhering moisture removed, no effect could be obtained, not even the slightest indications of attraction. I have varied the trials, using highly rarefied air at ordinary temperatures, and also condensed air deprived of moisture, with the same negative result. And I have been equally unsuccessful in substituting flame; unless the metallic points were in contact in the flame of the spirit-lamp, the passage of the electricity appeared to be completely interrupted. In very many experiments, employing the most active fish, if there were any visible space between the ends of the red-hot platina wire, I never witnessed the galvanometer in connexion with one wire affected, nor could obtain a shock. Reasoning on the subject, this perhaps is what might be expected, considering that the surface of the fish is a better conductor than air. One fact, however, which I had observed, afforded some encouragement to persist in the trials,—the fact that the torpedinal electricity passes through distilled water, which is a worse conductor of it than its own skin.

I thought it possible, that by insulating the torpedo on a plate of dry glass, and wiping its circumference dry, and smearing it with oil, that the galvanometer might be affected. But in this, too, I have been disappointed; not even in flame, when the interruption of the circle has been only just visible, has any effect on the instrument been produced.*

Mr. Farady, in the Third Series of his Experimental Researches on Electricity, states that he has little or no doubt, were Harris's electrometer applied to the torpedo, the evolution of heat would be observed.† I have made very many experiments on this subject, completely establishing Mr. Farady's anticipation. The instrument employed was similar to that described by Mr. Harris in the Philosophical Transactions for 1827, differing merely in the wire passed through the small globe being exceedingly

* Since the above remarks were first made, M. Matteucci has obtained sparks from the torpedo, and Mr. Farady from the Gymnotus, —not indeed directly, as Mr. Walsh was supposed to have done, but by means of a "magneto-electric coil," the object of which, it has been hypothetically said, is to prevent the electrical principles from neutralizing themselves directly through the conducting matter adjoining; and to force them to re-unite at a distance, by traversing the thin stratum of air in which the spark was taken. Bibliothèque Universelle de Genève, June, 1836, p. 387. The preceding part of this note was written in December;—I have since learned (now January 26, 1839,) that a spark has been taken direct from the Gymnotus, in the Adelaide Gallery, and that it was accidentally observed in making an experiment, instituted with a different object in view. My authority for this singular occurrence, and one so difficult of explanation, is the very intelligent superintendent, Mr. Bradley.

† Philosophical Transactions, 1833, p. 46.

fine, and of platina, formed after Dr. Wollaston's method;* in having a small stop-cock for regulating the height of the spirit in the stem; and in using as small a quantity of spirit as possible. The small globe or bulb of the thermometer was defended from the variable temperature of the surrounding air, by being included in a wooden box. The delicacy of this instrument was so great, that the spirit in the stem was not only moved by a single spark of the electrical machine, but even very distinctly by the electricity of a single voltaic combination, composed of a copper and zinc wire, the former 1-25th of an inch in diameter, the latter 1-50th, excited by dilute sulphuric acid.

This instrument was strongly affected by active fish, and even distinctly by weak ones;—indeed, occasionally, when it formed part of a circle in connexion with a galvanometer, I have seen it affected alone, the galvanometer affording no indication of the passage of the electricity. Using two air-thermometers of the same construction, each connected with the wires for contact at one end, and with the galvanometer at the other, the heating effect of the electricity of the torpedo has been apparently diminished, and even more distinctly diminished on adding to the circle another link of very fine platina wire. And at the same time its influence on the galvanometer has been diminished, and its power of imparting permanent magnetism to a needle

* Philosophical Transactions, 1813, p. 114.

placed in a spiral, both forming part of the circle. When heat has been applied to the entire link of platina by means of a spirit lamp, so as to render it red hot, the diminution of effect disappeared; and equally so, as well as I could judge from many experiments, whether acting on the thermometer, the galvanometer, or the needle in the spiral.

It appeared not improbable that a short portion of very fine platina wire might be ignited in the passage of the electricity of the torpedo. I have made several experiments to ascertain this, but I have never witnessed the effect, even in perfect darkness, and using fish, the discharge of whose electricity at the same time converted a needle into a tolerably powerful magnet; the needle having been put into a spiral connected with the fine wire, so as to afford a test of the strength of the electricity. This want of ignition may at first view seem contrary to the effect on the thermometer; but, perhaps, it ought not to be considered so, taking into account the rapid manner in which the heat evolved in the fine platina wire must be carried off by the adjoining compound wire of platina and silver.

The experiments which I have made on the electricity of the torpedo as a chemical agent, have been of a satisfactory kind. A small glass globe, of the capacity of about half a cubic inch, was used for holding the fluid to be acted on; and fine wires communicating with the contact-wires, were in-

troduced into it, through a perforated glass stopple, and they were cemented with sealing-wax along their whole course in the vessel, excepting at their points. By means of this little apparatus, I first tried the effect of a small active fish on a strong solution of common salt; the terminal wires were of silver. The contacts were made on the upper and under surface of the fish in the usual manner; minute bubbles of air collected round the point communicating with the under wire, but none at the other point. After an interval of some hours, fine gold wires were substituted for the silver wires: now gas was evolved from each extremity, but in largest proportion and in smallest bubbles, from the point connected with the under wire.

The next experiment was made on a strong solution of nitrate of silver; the terminal wires were of gold, the effect was distinct; the extremity of the under gold wire became black, and only two or three bubbles of air arose from it; the extremity of the upper gold wire remained bright, and it was surrounded with many bubbles of air. A similar experiment was made on a strong solution of superacetate of lead, and with results which were similar; but the effects appeared to be produced with greater difficulty; they were not distinct till the fish had been much irritated and seemed to put forth all its energy.

These electro-chemical experiments on the torpedo, were some of the earliest which I instituted on

its electricity; the notes of the first successful one bear the date of the 9th of September, 1831. In the autumn and early winter of 1833, I instituted several other trials which were amply confirmatory of the preceding. In these latter experiments I did not, as in the former, coat with sealing-wax the wires introduced into the fluids, leaving the points only exposed. Though the wires were naked, and in every instance, introduced more than a quarter of an inch into the fluid, and the distance was at least a tenth of an inch, yet satisfactory results were obtained. Using either a saturated solution of common salt, or a mixture of equal parts of sulphuric acid of commerce, and water, and platina or gold wires, gas was given off round each wire under the influence of the discharge of the electricity of an active fish, one contact wire being applied to the under surface, and the other to the upper surface of the torpedo. When steel needles were used with the salt water, then gas was disengaged only from the one in connexion with the under surface of the fish, the other needle becoming oxidated. Using a strong solution of nitrate of silver, and gold wires, silver was precipitated only on that in connexion with the under surface; employing strong nitric acid and platina wires, gas was given off from one only, that in connexion with the upper surface; using a solution of iodide of potassium and starch,* the iodine in combination

* When starch, in powder, is added to a saturated, or nearly saturated, solution of the iodide of potassium, a transparent gelatinous

with the starch, as indicated by the discoloration, was precipitated round the same wire.

Even the decomposition of water has been effected, when the circle has been interrupted by four portions of the solution of common salt, contained in small tubes, with two needles in each, the needles in one connected with those in the other, and at the same time with the galvanometer, — a spiral holding an unmagnetised needle, and an air thermometer. And simultaneous with the chemical decomposition, the needle in the galvanometer has been moved, and the spirit in the air thermometer has been raised, and the needle in the spiral has been magnetised.

Mr. Walsh, from his experiments, inferred that the two sides of the torpedo are when in action in opposite electrical states.* The results just described appear to prove that its under surface corresponds to the copper or negative extremity of a voltaic battery, and its upper surface to the zinc or positive extremity.†

mass is formed. This I have used in my experiments; a single combination of copper and zinc wire, acted on by very dilute acid, occasions, in this compound, a precipitation of iodine. The iodine is precipitated round the negative wire, or that connected with the copper. The effect is the same, whether platina or silver is made to convey the electricity into the saline solution. The decomposition probably is the effect of the hydrogen disengaged; it is not an immediate electro-chemical effect.

* Philosophical Transactions, Abridged, vol. xiii. p. 475.

† In my first paper on the torpedo, a mistake was inadvertently made relative to the electricity of the opposite surfaces of the torpedo, which has been pointed out by M. Becquerel, in his very able work on electricity.

To ascertain, if they preserve the same relation to each other, when the fish is made to act on the multiplier, and on the needle in the spiral, the following experiments were made. Successively at different times, with the same fish, and also with different torpedos, comparative experiments were tried on the course of the needle in the multiplier, when affected by the electricity of the fish, and by that of a couple of very small plates of copper and zinc immersed in a weak acid. In every instance the wire communicating with the under surface of the torpedo, was found to correspond in its effect with the copper plate, and that with the upper surface with the zinc plate; and whether one wire was in communication with the under surface of the fish, and the other with the upper, or the former with the copper plate, and the latter with the zinc plate, the deviation of the needle was in the same direction; its south pole turned to the west, and of course, its north to the east; and if the lower contact wire was made the upper, the effect on the deviation of the needle was identical with a change of the plates.

I have found the same uniformity of result in the polarity imparted by the torpedo to a needle in the spiral wire; the extremity of it, nearest the under surface in the circle, has always acquired southern polarity, and the other extremity of course northern.

Besides the preceding results, I have obtained some others, of a miscellaneous nature, which, in consideration of the obscurity of the subject, and its

novelty in many points, I do not think it right to withhold. By connecting the spiral with the multiplier, and charging the former with as many small needles as it could hold, namely eight, I ascertained that a single discharge of the electricity of an active fish moved the needle in the multiplier powerfully, and converted all the needles into magnets; and each of them, I believe, was as strong as if one only had been used.

Using two spirals charged with needles, one connected with one end of the multiplier, and the other with the other end, the effects of the discharge were similar to the preceding, both on the needle of the multiplier, and on the needles in the spirals. In two instances the needles in the spiral connected with the upper surface, were most powerfully magnetised; and in one instance, the effect was greatest on the needles in the lower spiral. In this last instance, nine needles were acted on in the under spiral, and six in the upper: the fish which produced the effect was, with one exception, the smallest that I had ever used.

In a few experiments on metallic conductors, the effect of the electricity of the torpedo on the galvanometer appeared to be much the same, whatever metals were used, and whether rusty or bright, provided the junctions were bright. The mass of metal appeared to have more influence; the effect, as might be expected, diminished with the increase of the mass; thus, when a poker weighing about

two pounds formed part of the circle, the effect on the electrometer, though distinct, was less powerful than when it was omitted; and when a large copper coal-scuttle was substituted for it, the effect was still more diminished, the deviation of the needle being only just visible. Extension of surface, as in the instance of increased length of wire, had a sensible modifying effect: thus, in an experiment in which about a thousand feet of wire were used (formed of three pieces, two about one-fiftieth of an inch in diameter, the third piece considerably finer), the motion of the needle was decidedly slower than when a short length of wire was employed, though the space traversed by the needle was not perceptibly different. I shall notice only a few of the experiments which I have made on imperfect conductors.

When I have held the contact-wire in the palm of each hand wetted with salt water, and have touched with the fore-fingers the upper and under surface of a torpedo, I have felt its shocks distinctly; but in no instance when the multiplier has been connected with the wires has it been affected; and when the spirals have been connected with them I have once only seen the needles in them converted into magnets. This effect accompanied a very smart shock from a young active fish, about six inches long, just taken.

When the touching ends of the contact wires have been covered with leather soaked in salt water, or

with cotton thread, all the effects of the fish, as might be expected, were witnessed, as if these imperfect conductors had not intervened ; the shock was felt by the hands holding the wires, needles in the spirals were magnetised, and the multiplier was moved.

When a cotton thread, soaked in salt water, or in a strong solution of salt, was interposed beyond the contact-wires, both the power of affecting the multiplier and of giving polarity to the needle in the spiral, was arrested ; and this was uniformly the result in a considerable number of experiments made with three different fishes, of which two were very active, and with perfect conductors free from this interruption, produced both effects readily. But the power of giving a shock was not equally arrested ; for on removing the multiplier and spirals, and holding with the wet fingers the wires attached to the moist cotton thread, the shock was several times distinctly felt on stimulating the fish. The space of cotton thread between the wires was about one-tenth of an inch, and to secure its perfect humidity or wetness, it was enclosed in a glass tube, with corks at each end, through which the wires passed.

When the apparatus, already described in noticing the chemical effects of the torpedo, was substituted for the wet cotton thread, the tubes being filled with a strong solution of salt, the multiplier was affected, and gas was given off at each of the points of the gold wires; and when steel needles were used,

a free current of gas rose from the point connected with the under contact wire, and not a particle from the other point. In these experiments, there were interposed at the same time the chemical apparatus, one on each side ; the spiral, one also on each side ; and the multiplier intermediate; and there were necessarily many junctions of wires. And in an after experiment, in addition to these, an air thermometer (Harris's Electrometer) was introduced, and very distinctly affected. I scarcely need add that in an experiment made expressly to ascertain this point, the shock of the fish was felt beyond the saline solution ; for it had been previously proved by the experiments of Mr. Walsh, that salt water, even in a long circuit of imperfect conductors, has a power of transmitting the electricity of the torpedo.

The tests or indications of the electricity of the torpedo, at present known, are six in number ; namely, the physiological effect, as the sensation it imparts is sometimes called ; the chemical effects, as the precipitation of iodine, the decomposition of water, &c. ; its effects on the thermometer, on the galvanometer, and on steel in the spiral. These different tests, in point of delicacy, I am inclined to believe, are in the order in which they are enumerated. That the two first should be placed highest, and that sensation should have the precedence, the experiments which I have made appear to prove independently of all analogy.

When the human body has formed part of a

circle of communication between the two opposite surfaces of a torpedo, and also a chemical apparatus with platina wires and the solution of iodide of potassium and starch, the shock experienced by the hands has been strong, and the chemical effect either null or slight; no gas appearing when a strong solution of salt has been used, and no precipitation of iodine occurring unless the platina wires were very nearly in contact, and the fish energetic.

When, besides the human body, and the chemical apparatus, the galvanometer has been introduced into the circle with the air thermometer and the spiral, the shock has been experienced as if it had been received direct from the fish; but I have never witnessed at the same time any other effect.

Not taking the human body into the circle, in trials on fish of very feeble electricity, I have witnessed the precipitation of iodine, when neither the air thermometer, nor a delicate galvanometer with a double needle has been affected.

The same kind of evidence has been obtained of the thermometrical test, being next in point of delicacy, inasmuch as I have seen the air thermometer affected by a fish which had no influence on the galvanometer, in connexion with the wire of the thermometer.

That the needle in the spiral is the least delicate test, does not require to be insisted on. The electricity of a torpedo, of almost feeble energy, has been equal to produce all the effects alluded to at once, excepting sensation, as already explained, and

excepting the imparting permanent magnetism to the steel needle. The last effect, as might be expected, has required greater force; a moderate force, however, has been sufficient when a very slender needle has been used, and a spiral of fine wire closely coiled, only just capable of receiving the needle.*

The few experiments which I have made on the torpedo, analogous to those instituted by Mr. Todd, described by him in the Philosophical Transactions for 1816, have afforded very similar results. When the brain has been divided longitudinally, the fish has continued to give shocks. When the brain has been entirely extracted, the fish instantly lost this power, though the muscles generally continued to act powerfully;—nor could any shock be procured in this instance, either by puncturing with a sharp instrument the electrical nerves, where they quit the cavity of the cranium, or where they enter the electrical organs, just after passing between the branchial cartilages. On one occasion, however, it may be mentioned, that when a small portion of the brain was accidentally left, contiguous to the electrical nerves of one side, and with which they were connected, then the fish, on being irritated (the remaining portion of brain was touched) gave a shock to an assistant who grasped the corresponding electrical organ.

* The spiral wire above alluded to, is fine copper-wire gilt, having about 300 convolutions to the inch; an inch of it weighed four-tenths of a grain.

M. De Humboldt states, that the shock of the torpedo may be procured by touching with the finger or hand, one surface only of the fish.* The experiments which I have made expressly on this point have led me to a different conclusion, namely, that it is requisite to touch the opposite surfaces of the electrical organs or organ, or a conductor or conductors connected with them, to receive a shock. In very many instances that I have irritated torpedos by pressing with the finger on different parts of the back, as the upper surface of the electrical organs, and on the margin of the pectoral fins, however much the fish were irritated, I never had any sensation excited by the electricity which there was reason to believe was discharged; though immediately after, on touching the two surfaces, irritating only the upper, shocks were received. On some few occasions, I have perceived a shock, when apparently only one surface of the fish was touched; but I believe in those instances the discharge took place through the water.† In corroboration I may mention, that in experiments in which one surface only has been touched and irritated, the fish itself appears to make an effort to bring, by muscular exertion, the border of the under surface (the upper being pressed on) in contact with the offending body;

* Annales de Chimie et de Physique, tom. xi. p. 430.

† The most remarkable example of the kind of which I have any note, was that of a young torpedo, which gave slight shocks to the hand on which it was supported, whether just under the surface of the water, or just after it was taken out of the water into the air.

and this I have witnessed as distinctly in the fœtal fish as in the adult; clearly showing that the effort is instinctive. The conductor, which I suppose to be necessary for conveying the electricity, when a shock is felt without immediate contact, exists in salt water. The galvanometer has been affected when the two extremities of it have been brought in contact, one with the back of the fish, and the other with the water, two or three inches from the fish. And, in one instance I experienced a shock, although I touched the water alone, close to a torpedo; it was in removing an active fish, by means of an earthenware dish, from one vessel into another; the hand that received the shock grasped the wet margin of the dish just as the torpedo entered it. I believe that the torpedo has the power of discharging its electricity in any direction it chooses. This inference is drawn from finding that when one hand, in contact with the opposite surfaces of the fish, is receiving shocks, the other hand immersed in the water close by, has received no shock. And, in confirmation of this, I may mention (and at the same time to show how the discharge is connected with the volition of the animal,) that when I have applied to the opposite surfaces of a torpedo copper plates, merely gently touching, joined together by a copper wire, and then irritated the fish with the contact wires in the usual manner, the galvanometer attached to the contact wires has been distinctly affected.

In my first communication to the Royal Society,

it is conjectured, that the mucus with which the torpedo is lubricated, may be a conducting medium between the two opposite surfaces. This was an erroneous view. It may serve that purpose to the surfaces individually. Seeing the error theoretically, I was led to examine the margin of the fins, and they have appeared to me to have less mucus adhering to them than any other parts of the fish, as if intended partially to insulate the electrical organs.

In that communication also, I stated my inability to account for my brother's not obtaining any positive results in his experiments on the torpedo. After reconsidering the subject, I am disposed to think it might have been owing to his using large fish, without the means of ascertaining their electrical activity, excepting by the shock. And, we have seen, that when the human body forms part of the circle of communication with a galvanometer, the latter is not affected in the passage of the electricity producing the shock; which may serve to explain his not having witnessed any effect on the instrument at Trieste. As regards the electrical energies of large torpedos, nothing is more uncertain. There appears to be no relation between the muscular and electrical power. I have seen strong vivacious fishes, which made great muscular exertions in the water, almost or entirely destitute of electrical power; and, on the contrary, I have seen others languid and moribund, which have exerted considerable elec-

trical power. Small fish are almost always active electrically, and they are greatly to be preferred as subjects for experiments: moreover, they have this advantage,—they can be kept alive a much longer time.* Mr. Walsh noticed, in the fish on which he experimented at Rochelle and the Isle of Ré, a retraction of the eyes of the torpedo, at the instant it exercised its electrical function. This I have not witnessed in the torpedos of the Mediterranean; nor indeed have I been able to associate any visible sign, any apparent movement of the fish, with the electrical discharge.

2.—*Observations on the Electrical Organs of the Torpedo, and on some parts of its structure connected with them.*

The peculiar columnar appearance of the electrical organs of the torpedo, and their great proportional size, the vast proportion of nerve with which they are supplied, the manner in which the columns are sheathed in tendinous fibres, have been dwelt on by all inquirers who have paid any attention to this fish; but I am not acquainted with any attempt to ascertain by experiment what is the exact nature of the substance of these organs, or the peculiar structure of which they are composed.

* The torpedo should be kept in an earthenware vessel, not a *wooden* one, which exercises some noxious influence on the fish: the water (the purest sea-water that can be procured) should be changed daily; and the coolest place should be selected for it,—where the sun never shines.

When I have examined with a single lens, which magnifies more than two hundred times, a column of the electrical organ, it has not exhibited any regular structure; it has appeared as an homogeneous mass, with a few fibres passing into it in irregular directions, which were probably nervous fibres.*

The specific gravity of the electrical organ, in comparison with that of parts of the fish decidedly muscular, is very low; including the upper, and under boundary of skin, I have found it 1·026 to water as 1·000. The specific gravity of a portion of the abdominal muscles of the same full grown fish was 1·058, and that of the thick strong muscles of the back close to the spine 1·065.

The loss of weight which the electrical organ sustains by drying, is greater than I have observed in any other part of the fish. I shall give the results of one trial; the statement will convey an idea of the bulk of the different parts of the torpedo, as well as of the proportion of solid matter which they contain. The subject of the experiment, procured fresh from the fish market at Rome, was 8 inches long, and across the widest part 5 inches broad. Entire it weighed 2065 grains. It was carefully

* Farther microscopic examination has confirmed the above observation. The fibres of the voluntary muscles, even after having been four years in spirits, exhibited the peculiar transverse striated appearance: no regularity of structure; nothing distinctly characteristic was visible in the tissue of the organ in question.

divided, and the different parts mentioned were found to weigh as follows in their moist state :—

Spleen - - - -	5·5 grs.
Pancreas - - - -	5·0
Testes - - - -	3·0
Kidneys - - - -	8·0
A pale cream coloured oval body close to left kidney -	0·25
A reddish oval glandular body attached to the large intestine*	0·5
Liver with gall bladder and ducts	105·0
Heart and trunk of pulmonary artery - - - -	3·0
Gills including branchial cartilages - - - -	53·0
Gullet - - - -	11·0
Stomach - - - -	65·0
Upper valvular intestine -	29·0
Lower intestine - -	5·0
Electrical organs - -	302·0
Head separated at first vertebra	165·0
Thorax consisting of cartilaginous case and muscles with pectoral fins attached -	670·0
Abdomen without its contents	440·0
Tail separated just below the anus	195·0

* This is found in all the cartilaginous fishes; it contains a cavity communicating with the intestine, and abounds in follicles; its function seems to be similar to that of the appendicula vermiformis of man.

By exposure to the heat of boiling water for about sixteen hours, the different parts were completely dried; their total weight was reduced to 322 grains, so that they had lost by drying 84.5 per cent.

The electrical organs now weighed	22 grs.
Head - - - - -	25
Thorax - - - - -	93
Abdomen - . - -	53
Tail - - - - -	36
Liver (abounding in oil) - -	43
Residue consisting of other organs, and extract of fluids which exuded during the drying - -	50

From the above loss of weight of the electrical organs in drying, they appear to consist of 7·28 matter *not evaporable* at 212 Fahrenheit, and of 92·72 water, taking it for granted that the loss sustained is owing merely to the evaporation of the aqueous part. I lay stress on matter not evaporable, because I believe that the solid contents of the moist organs are less, and that the water which they contain holds in solution various substances.

This solution may be obtained by cutting the electrical organs into small pieces, and placing them in a funnel. The fluid part slowly separates. What I have thus collected was slightly turbid, of a very light fawn colour, just perceptibly acrid; it

did not change the colour of turmeric or bitmus paper; a cloudiness was occasioned by dropping into it a solution of nitrate of silver, which was not completely redissolved by *aqua ammoniæ;* it was copiously precipitated by acetate of lead, and a cloudiness was occasioned in it by nitrate of barytes, and by corrosive sublimate. By evaporation it afforded a residue which deliquesced partially on exposure to a moist atmosphere, and had an acrid and bitter saline taste. The exact proportion of this weak solution of animal and saline matters I have not ascertained, and indeed it would be very difficult to determine with any accuracy, for only a small portion separates spontaneously; and if pressure is used, the fibres are broken, and the fluid expressed is mixed with a pulpy matter.*

When the electrical organs of the torpedo are immersed in boiling water, they suddenly contract in all their dimensions, and the columns from pentagonal, which they generally are, become circular. In my early experiments at Rome, they were rendered firmer by immersion for a few minutes, and the columns appeared to be tolerably distinctly fibrous and laminated, bringing to recollection the structure of the pile of Zamboni. Latterly I have not witnessed this effect; in a few seconds the

* The torpedo is used for food, in the Mediterranean, amongst the lower classes; but the electrical organs are rejected,—they are considered unwholesome.

tendonous fibres have been converted into jelly, and the columns have fallen asunder, having the appearance and consistence of a translucent very soft mucilage. To what this difference of effect may be owing, I am at a loss to conceive; perhaps the Roman fish were older than the Maltese; or the aqueduct water at Rome may be harder than the rain cistern-water of Malta.

On exposure to the air in a damp atmosphere, or by maceration in water, changing the water daily, the electrical organs undergo change more slowly than the parts distinctly muscular; in putrefaction and maceration they resemble less muscular fibre than tendinous fibre, which latter offers great resistance to both these processes. But I would not lay any stress on this quality of resistance, as it is extremely vague, depending on circumstances which it is difficult to appreciate, as every one must be convinced, who has compared the different degrees of rapidity with which different orders of muscles of man, and the larger mammalia, undergo change from putrefaction and maceration; for instance, the slowness with which the muscular fibres of the stomach and intestines alter, and the rapidity of change of the fibres of the heart and thick muscles.

Quitting the organs of the dead fish, I shall now notice the few observations which I have made on them before they have been deprived of their vitality.

The effect of the electricity of a small voltaic,

trough, the shock of which I could just perceive at the extremities of my moistened fingers, was very distinct on the voluntary muscles of a live torpedo, just taken from the water; but it did not appear to affect in the least the electrical organs. I could not perceive the slightest contraction of them in whatever manner the wires were applied, not even when a minute portion of integument was removed, or when one of the wires was placed in contact with a fasciculus of the electrical nerves. Even after apparent death, many of the parts decidedly muscular continued to contract under this stimulus, especially the muscles of the flank, and the cross muscles of the inferior surface of the thorax, and the heart; indeed this latter organ, two hours after it had been removed from the body, and had ceased to contract spontaneously, renewed its contractions under the galvanic influence.

Other stimulants have been applied to the electrical organs, and with the same negative result. Even when punctured, or incised (a portion of their skin having been removed, which appears to be very sensitive) no indications whatever were witnessed of their substance being either sensitive or contractile.

Reflecting on the facts and observations which I have just detailed, it appears to me very difficult to resist the conclusion, that the electrical organs of the torpedo are not muscular, but columns formed of tendonous, or nervous fibres, distended by thin gela-

tinous fluid. Their situation too, surrounded by, and exposed to the pressure of powerful muscles, shews, that if condensation is required for the exercise of the electrical function, they may experience it without possessing any muscular fibres in their own substance. The arrangement of the muscles of the back and of the fins, and of the very powerful cross-muscles, situated between the under surfaces of the electrical organs, are admirably adapted to compress them. Without entering into any minute anatomical examination of these muscles and their uses, it is only necessary to compare them in the torpedo, and in any other species of Ray, to be convinced that they are adequate to, and probably designed for the effect mentioned.

Mr. Hunter,* in his account of the torpedo, describes the columns of the electrical organs as composed of cells containing a fluid, divided by horizontal partitions which he was able to count. This structure seems very probable, and in the specimens I dissected at Rome, I saw what I fancied an approach to it; but I have never witnessed it in a satisfactory manner in the fresh fish. Mr. Hunter inspected large fishes which had been preserved in spirits. The partitions of the columns in them might have been more visible (supposing them to exist) from the action of the spirit on the membrane, and from the greater size of the specimen: or they might have been formed after death in the spirits by

* Philosophical Transactions, 1773.

a slow deposition of the animal matter contained in the columns.

Next to the nature of the substance of the electrical organs, the electrical nerves have occupied my attention. Their three great trunks have been accurately described by Mr. Hunter; but this distinguished anatomist has very briefly noticed their distribution, which is curious, and deserving, I believe, of minuter investigation. I shall attempt little more than an outline of what I have observed in some dissections conducted with considerable care.

In examining the brain (vide Plate I, fig. 1.) proceeding from the anterior to the posterior portion, after passing the first, second, third, and fourth pair of nerves, or the olfactory, optic, motor, and pathetic* nerves of the eye, the fifth pair is seen issuing from the medulla oblongata, or posterior tubercle of the brain. After quitting the cranium (confining the description to one side) it proceeds upwards, divides into two large branches, which go to clusters of mucous glands, situated in the front of the head, and at the interior margin of the electrical organs, and they appear to be confined to these parts. The next pair, the first electrical, rises close to the preceding, just behind it, and in passing out of the cranium is firmly connected with it; and also where it passes out, a portion of medullary matter proceeds from it into a cavity filled with fluid in the cartilage adjoin-

* The nerves of the fourth pair are so very small and tender, that it is difficult to demonstrate them excepting in old and large torpedos.

ing, which there is reason to consider as the cavity of the organ of hearing, and the medullary matter the nerve of hearing. After this, in passing outwards, it divides into three small branches, and two large ones. Of the former, one proceeds to the gills, another to the adjoining muscles, and the third to the mouth. Of the great branches, one ascends, and sweeping round the margin of the electrical organ, is distributed to the mucous glands which abound there, and where some of its twigs inosculate with twigs of the former nerve. The other great branch, which is inferior, enters the electrical organ, and ramifies through its superior portion. The next pair of nerves, the second electrical, rises a little beyond the preceding. On leaving the cranium, it divides into two great branches; these, with the exception of nervous twigs supplying the adjoining branchiæ, are distributed entirely in the substance of the electrical organ, and ramify in all directions through its middle portion. The third electrical rises close to the last, divided only by a very thin plate of cartilage. The principal portion of it passes into the electrical organ, and ramifies through its inferior part. Besides, it gives off three small branches, which are sent to the adjoining branchiæ, to the gullet and stomach, and to the tail. The branch which supplies the stomach appears to be the principal nerve of this organ; it descends along the inner and inferior portion of the gullet, and ramifies in the direction of the great arch of the

stomach. The caudal branch (nervus lateralis) descends in a straight line under the peritoneal lining of the abdomen, and under the spinal nerves, without giving off a single branch, till it reaches the tail, in the muscular substance of which it is lost.

I have not yet been able to discover any connexions of the electrical nerves besides those pointed out. It is an interesting fact that the gastric nerves are derived from them. Perhaps superfluous electricity, when not required for the defence of the animal, may be directed to this organ to promote digestion. In the instance of a fish which I had in my possession alive many days, and which was frequently excited to give shocks, digestion appeared to have been completely arrested; when it died, a small fish was found in its stomach, much in the same state as when it was swallowed: no portion of it had been dissolved.

Though I have not found the temperature of the electrical organs higher than that of other parts of the fish, or the temperature of the fish generally different from that of the water in which it has been confined, yet it seems probable that as the branchiæ are liberally supplied with twigs of the electrical nerves, there may be some connexion between its respiratory and electrical function; and I venture to offer the conjecture that, by means of its electricity, it may have the power of decomposing water, and of supplying itself with air when lying covered with mud or sand in situations in which it is easy to con-

ceive pure air may be deficient; and in my experiments, I have often fancied that I have witnessed something of the kind; after repeated discharges of its electricity, the margin of the pectoral fins has acquired an appearance as if very minute bubbles of air were generated in it and confined.

Besides the electrical nerves, there is a fasciculus of nerves deserving attention, of great magnitude, formed by the junction of the anterior and posterior, or upper and under cervical nerves; of the former, about seventeen on each side; of the latter, about fourteen.* It makes its appearance as one trunk just below the transverse cartilage which is interposed between the thorax and the abdomen. It sends a recurrent branch to the muscles and skin of the under surface of the thorax, but its main trunk ascends along the inner margin of the pectoral fin, and is distributed through it. On this fasciculus the sentient and motive powers of the parts connected with the electrical organs seem to depend.

The electrical nerves at their origin are enveloped in a very thick fibrous sheath. As the branches subdivide in the substance of the organ, the neurilema becomes thin and semi-transparent. On examining a minute branch with a powerful lens, its internal or medullary substance is not seen in a continuous line,

* Towards the origin of the spinal cord, there is a small space, from the under surface of which, six nerves arise, three on each side; but none from the upper surface,—whence the difference of number noticed in the text.—Vide Plate I. fig. 2.

but interrupted, as it were dotted, as if the sheath contained a succession of portions with a little space between each.*

In the anatomical structure of the torpedo, the mucous system forms a very conspicuous part; it consists of several clusters and chains of glands distributed chiefly round the electrical organs, at different depths beneath the cutis, and of strong transparent vessels of various lengths and sizes opening externally on the skin, for the purpose of pouring out the thick mucus secreted by the glands, and destined for lubricating the surface. This system has not been noticed by Mr. Hunter, and it has been but imperfectly described by Lorenzini.† Though it is not peculiar to the torpedo, it is much more strongly developed in this fish than in any other species of Ray with which I am acquainted; and the situation of the glands, and the distribution of their vessels are different. Whether it is concerned in any way with the electrical function of the torpedo, is deserving of consideration. That it is thus con-

* An after examination, using Ross's microscope, and his object-glass of one-eighth of an inch focal distance, has not confirmed the above. Comparing a fibril of an electrical nerve and of a spinal nerve, taken from a fish which had been preserved in spirits four years, there appeared to be no material difference in their minute structure: each appeared irregularly fibrous,—that is, composed of fibres not perfectly continuous or exactly parallel, connected by a delicate net-like tissue.

† Osservazioni intoruo alle Torpedini fatte de Steffano Lorenzini Fiorentino. 4to. Firenze, 1678. Vide Plates II. and III.; farther on, a more particular account will be given of the structure.

cerned in some way, seems to be indicated not only by the situation of these glands between and surrounding the electrical organs, but still more so by the manner in which they are supplied with nerves, either from the first electrical, or from the fourth pair, which is connected with that nerve. As the thick semi-transparent mucus which the glands secrete is probably a better conductor of electricity than the skin alone or than salt water, this mucous system may serve as a medium of communication, not for the opposite surfaces of the electrical organs, but for each surface individually, included within the line of the system referred to.* I shall mention some results which are favourable to this idea.

When one contact-wire was placed underneath an active torpedo, just anterior to the mouth, and the other at the extremity of the back, out of the circle of the mucous apparatus, the shock of the fish had no effect either on the multiplier, or on needles in the spiral. But when the upper contact-wire was made to touch the back of one electrical organ, the under wire being placed as in the preceding experiment, then both effects were simultaneously produced; and they were also produced when the two wires were

* Some comparative experiments, which I have made, seem to indicate that the mucus of the torpedo is a better conductor than sea-water; when the hands were smeared with this mucus, or when a portion of the fresh skin of a torpedo, with its natural mucus adhering to it, was wrapped round the ends of the contact wires, by which they were held, the shock received appeared to be stronger than usual.

brought very close to each other, one being kept as before, and the other moved immediately over it, in front, each about a quarter of an inch from the margin, and not connected with the electrical organs, except by the common integuments and this mucous apparatus. It is worthy of remark that this little space in front, intermediate between the two electrical organs, so abounding in glandular structure, and so amply provided with nerves, appears from experiments to possess very little sensibility. This was denoted in these trials, in which the fish, though exquisitely sensible of pressure on the margin of the pectoral fins, seemed indifferent to it when applied in front, as if the fourth pair, which supplies this part, were destined rather for secretion than for the purpose of sensation.

The connexion between the electrical nerves, and the mucous system, even more remarkable than between the former and the stomach, may perhaps warrant the conjecture that the electrical function may not only be aided by, but also aid the secretion of mucus; and that, as was supposed in regard to the stomach, when the electricity is not employed, in repelling an enemy, in violent efforts, it may be exercised gently in increasing the activity of these glands. In support of this notion it may be mentioned, that in the fishes which I have kept, in which digestion was arrested, the secretion also of mucus appeared to be stopped, or considerably diminished.

Mr. Hunter, from the examination of a torpedo whose vascular system was injected, states that the electrical organs of this fish are abundantly supplied with blood-vessels. From what I have witnessed in the living fish, and the fresh fish recently dead, I am compelled to conclude that the quantity of blood which circulates through them is very inconsiderable. The blood-vessels which pass into them with the electrical nerves are small; the organs are colourless, and very few branches carrying red blood are perceptible, extending through them. The integuments of these organs, and the pectoral fins, and lateral clusters of mucous glands are indeed abundantly supplied with blood-vessels; the contrast of the vascularity of these parts, and of the electrical organs, is so strongly marked as to suggest the idea, that the latter can possess very little ordinary vital activity, and that in accordance, with the common analogies of living parts, they must be rather passive than active.

Some circumstances pertaining to the integuments of the electrical organs are deserving of notice. I have found that the skin covering these organs above is not only more coloured, but also thicker than that covering them below, and more vascular, and surrounded by more powerful muscles, and supplied with a greater quantity of mucus; whilst the under surface appears to have a larger proportion of subcutaneous nerves. This difference of structure in the two surfaces of the electrical

organs is probably somehow connected with their opposite electrical states.

I may here notice another peculiarity of organization in the torpedo, which came under my observation in seeking, though unsuccessfully, for the great sympathetic or the analogous ganglionic nerves, which Cuvier asserts to exist in the cartilaginous fishes.* The peculiarity alluded to is represented in Plate I. fig. 3. It has very much the appearance of a nervous ganglion, but is in reality a blood-vessel, enlarged into a little bulb, lined with a reddish substance, like muscular fibre, giving the idea of a small heart. It is situated one on each side of the aorta, from whence it proceeds, just below the great nervous fasciculus which supplies the pectoral fin, and the arterial branch derived from it is lost in this fin. If it be muscular, as its appearance denotes, its functions may be to aid in propelling the blood into the pectoral fin, and perhaps into the electrical organ.

3.—*Theoretical Remarks.*

The experiments which I have detailed on the electricity of the torpedo, confirm those of Mr. Walsh, made in 1772, shewing its resemblance to common electricity. They moreover show, that like common electricity, and voltaic electricity, it has the power of giving magnetic polarity to iron, and of producing certain chemical changes. In

* Hist. Nat. des Poissons, tom. i. p. 438.

these its general effects, it does not seem to be essentially peculiar, but as much allied to voltaic electricity, as voltaic electricity is to atmospheric, or atmospheric electricity is to that produced by contact or friction. When we examine more minutely its phenomena or effects, in relation to these different kinds or varieties of electricity, certain points of difference occur.

Compared with voltaic electricity, its effect on the multiplier is feeble; its power of decomposing water and metallic solutions is inconsiderable; but its power of giving a shock is great, and so also is its power of magnetising iron.

Compared with common electricity, it has a power of affecting the multiplier, which under ordinary circumstances, common electricity does not exhibit; its chemical effects are more distinct; its power of magnetising iron,* and giving a shock appear very similar; its power of passing through air is infinitely less; as is also its power of producing heat.

There are other points of difference; I allude chiefly to the results obtained in the experiments already described, in which the metallic communi-

* There is this difference when two spirals are used, one connected with the inside of a Leyden jar, and the other with the outside; a needle in each, similarly placed, acquires opposite polarities, the north pole in one being where the south pole is in the other; whilst in the instance of the torpedo they accord, so that a line of needles, passing from one side of the electrical organ to the other, would exhibit a succession of similar poles.

cation was interrupted by a strong solution of salt. In this instance the full power of the fish appeared to pass; water was decomposed, a shock was received, needles were magnetised, and the multiplier was affected. When the same experiment was made on the electricity excited by the small voltaic combination of a single plate of copper and zinc, each less than an inch in length, and half an inch in breadth, immersed in an acid, neither water was decomposed, nor was the multiplier affected. When it was made on the electricity of the electrical machine, by means of a Leyden jar, all the effects were witnessed, excepting the motion of the multiplier, and the order of succession of poles in the needles magnetised in the spirals. How are these differences to be explained? Do they admit of explanation similar to that advanced by Mr. Cavendish, in his theory of the torpedo, and which has recently been ably advocated by Mr. Faraday? or are they more in accordance with the idea which my brother was disposed to adopt, that the electricity of the torpedo and of the other electrical fishes is peculiar,—a power *sui generis?* or, lastly, may we suppose, according to the analogy of the solar ray, that it is not a single power, but a combination of powers?

The first opinion, which is commonly received, is supported by the majority of facts detailed in the preceding pages. The circumstance principally hostile to it, at least in appearance, is the interruption of the torpedinal electricity by the smallest quantity

of air, and its want of the power of attraction and repulsion in the air.

These peculiarities are seemingly in favour of the second opinion, that the electricity of the torpedo is specific and peculiar. But till the opposite surfaces of the electrical organs can be perfectly insulated, so that no easier mode of communication is afforded than through air, they can hardly be considered of much weight.*

The third opinion may be indulged in as an hypothesis; as a guide to research it may not be useless. It applies, however, almost as much to other varieties of electricity as to that of the torpedo; all of which, it is possible, may be compounded, or owe their various effects to the union of several powers, or ethereal fluids, and their peculiarities compared one with another, to the predominance in various degrees of these fluids. What is known of the solar ray is not unfavourable to such an opinion; and the history of physical science, in relation to elementary ponderable matter, may give rather encouragement to the notion.

As regards the mode of production, or the cause of the electricity of the torpedo, it is unavoidably enveloped in great mystery. Like animal heat, and the light emitted by certain animals, and I may add,

* In the experiments in which I attempted to insulate the surfaces, by means of oil, the probability is, that I failed, and that a communication continued, if not by the outer surface of the skin, at least by its inner; indeed, the attempts to insulate these organs, in the manner desired, seems to be almost hopeless.

the secretions of animals generally, it appears to be the result of living action; and connected with a peculiar and unusually complicated organization. All the attempts have been vain, which I have made, directed to obtain electrical excitement in the fish, after it had been deprived of life.

The observations which I have detailed relating to its anatomical structure, show a complicated adaptation of parts—nerves of unusual magnitude ramifying between apparently insensible columns, saturated with a bad conducting fluid; muscles surrounding these columns, and fitted to compress them; and a system of mucous glands and tubes adjoining, well adapted to be the medium of electrical communication in the surfaces to which they belong.

When we consider this structure, it is an easy matter to trace rude analogies between it and the pile of Volta, or between its columns, and a battery of Leyden jars, such a battery as was formed by Mr. Cavendish, for imitating the electricity of the torpedo, composed of a large number of jars of very thin glass feebly charged. But these analogies seem to help very little, if at all, towards the solution of the great difficulty; the question remains unanswered, what is the cause or source of the electricity? Here analogy fails entirely; none of the ordinary modes of excitement appear to be at all concerned—neither friction, nor chemical action, or change of temperature, or change of form. Let us consider for a moment a small torpedo in an active state. The

smallest which I have employed in my experiments weighed only 410 grains, and contained only 48 grains of solid matter; its electrical organs weighed only 150 grains, and contained only 14 grains of solid matter,—for to this they were reduced by thorough drying. Yet this small mass of matter gave sharp shocks, converted needles into magnets, affected distinctly the multiplier, and acted as a chemical agent, effecting the decomposition of water, &c. *A priori*, how inconceivable that these effects could be so produced! This fish was about ten days in my possession, during the whole of which time it ate nothing, and its bulk was hardly sensibly altered; and every day it exercised its electrical powers, and to the last they appeared almost as energetic as when it was fresh from the sea. This adds, if possible, to the difficulty of explanation. That this mysterious function is intimately connected with the nerves, and in a manner more striking than all ordinary secretions, is manifest. Beyond this conclusion, all is darkness. We have not, as we have in the doctrine of animal heat, advanced another step—we have not been able to connect it with changes in the electrical organs, as analogous to known sources of electricity, as the changes which take place in the lungs in respiration are to the known sources of heat or combustion. The attainment of this step is a great desideratum, and beyond it, probably, we shall never be able to proceed.

Without reverting to the conjectures, which in pas-

sing I have offered on the subserviency of the electricity of the torpedo in an auxiliary manner to digestion, respiration, and the secretion of mucus, I may remark that its chief use appears to be for purposes of defence—to guard it from its enemies, rather than to enable it, according to vulgar opinion, to destroy its prey, and provide itself with food. Small smelts, which were kept in the same vessel with torpedos, appeared to have no dread of them, and I believe they fed on their mucus. And, in an experiment in which, in a confined space, an active torpedo was excited to give shocks, a smelt which was with it was evidently alarmed, and once or twice exposed to the shock, leapt nearly out of the vessel; but was not injured by the electricity. In confirmation, I may add that the electric power of the young fish, which most requires it for its protection, is, as already observed, proportionally very much greater than that of the old, and can be excited without exhaustion and loss of life, much more frequently. After a very few shocks, most of the old fish which I have had have become languid, and have died in a few hours; whilst young ones from three to six inches long have remained active during ten or fifteen days, and have never failed to shew the effects I have described.

4.—*Of the other Electrical Animals compared with the Torpedo.*

The number of animals of this description hitherto discovered and well authenticated is very small; in brief, the Gymnotus Electricus and the Silurus Electricus, besides the Torpedo, are the only ones, the existence of which is certain and free from all doubt;* and to these I shall confine the few remarks

* In the list of electrical fishes, two others are commonly introduced—the so named Tetraodon Electricus and Trichiurus Electricus; but neither on good authority. The information on which the existence of the first has been received, is most scanty and imperfect. The narrative has a good deal the manner of fiction. It is contained in the Philosophical Transactions for 1786, in a letter from Lieut. William Paterson, of the 98th Regiment, to Sir Joseph Banks, accompanied with a drawing. The writer states, that when accompanying his regiment to India, in the Island of Johanna, where they touched, he met with a new species of electrical fish, of the genus Tetraodon, as he supposes, in cavities on the shore in the coral rock, and that in handling one he received a shock. He says, he saw several of them, and that the water in which they were was about 55 or 60° of Fahrenheit, although the island is within the tropics. (sea water of 55° or 60° in lat. 12° 13' south!)

The reality of the other fish, Trichiurus Electricus, is more than doubtful, although so called by Lacépède and by Shaw. The author of the "Supplement on the first order of Fish," to the English Translation of Cuvier's "Regne Animal," remarks, "that the characters and properties announced with so much assurance, rest only on a bad figure given by Nieuhoff, and a transposition of a part of the text, which has no reference to the figure, nor to any trichiurus." Vol. x. p. 348. How true is the remark, in natural as well as in civil history, that "men love to complete what is imperfect, and to realize what is imaginary!"

which I propose to offer relative to their comparative powers and structure.

All these fishes have certain points of resemblance. Independent of their electrical powers, they are peculiarly defenceless; their skin is soft—a true mucous membrane, although external, totally destitute equally of scales and of spines. Each possesses a supplementary organ, with which their electrical function is connected; and in each the nerves supplying these organs are of extraordinary magnitude.

The electrical organs of the respective fishes bear to each other in their structure a general, but not close, resemblance. The organs of the torpedo are distinctly columnar—the columns very numerous; those of the gymnotus consist of septa, or of flat partitions with cross divisions between them; whilst those of the silurus are neither distinctly columnar, nor cellular, being formed as it were of a delicate net-work of tendonous fibres, retaining interposed a soft gelatinous matter, somewhat similar to that contained in the fibrous columnar or cellular structure of the organs of the former fishes.

The nerves supplying the organs are peculiarly deserving of remark. It is a curious circumstance, that in each instance, their source is different.

In the torpedo, we have seen that they are derived exclusively from the brain, from its posterior tubercle.

In the gymnotus electricus, they do not appear to come at all from the brain, but entirely from the

spinal cord. This was the result of Hunter's dissection: he says, "the organ is supplied with nerves from the medulla spinalis, from which they come out in pairs between all the vertebræ of the spine."

In the silurus electricus, according to the researches of Rudolphi, they appear to have a double source: those supplying the outer organ being derived from the fifth pair; those supplying the inner, from the intercostal.

Moreover, a marked difference appears in the proportional magnitude of the nerves in the several instances. In the torpedo they appear to be largest; in the silurus smallest. Hunter says, "if all the nerves which go to it (the electrical organ of the torpedo) were united together, they would make a vastly greater chord, than all those which go to the organs of this eel," (the electrical gymnotus.)* Their comparative smallness in the silurus, I have satisfied myself of by my own observations, made on a specimen, which through the kindness of the American consul at Malta, I procured from Egypt, and which is now deposited in the museum at Fort Pitt.

As regards electrical power, it is commonly believed, that it does not accord with the proportion of nerve; that it is feebler in the torpedo than in the silurus, and greatest in the gymnotus.

In relation to the shock or sensation which these fishes impart, the opinion probably is true. What

* Philosophical Transactions for 1775.

I have heard in conversation from that distinguished traveller and naturalist, Dr. Rouppell, would seem to indicate that the shock of the silurus is more energetic than that of the torpedo; and that that of the gymnotus is the most energetic, seems not to admit of doubt, considering the well authenticated accounts we have of its effects on large animals, such as the horse, especially as related by M. De Humboldt.

But it does not, therefore, necessarily follow, that the power is in other respects proportional; for instance, as a chemical, heating, or a magnetising agent. Certain forms of electricity differ in these manifestations, and why not the electrical power of these fishes?

The question can only be determined by actual experiments. The arrival of a gymnotus in England, with liberal permission from the proprietors of the Adelaide Gallery of Practical Science to make trial of it, has enabled me to do this, in a partial manner. It was on the 29th of November last, that I made the trial, when I was assured by Mr. Bradley, that the fish was, as it appeared to be, in excellent health. For the sake of comparison, I used the same apparatus as I had employed at Malta on the torpedo, and in the same manner. The first experiment attempted was on the galvanometer. The effect of the electrical discharge of the fish was very decided; the needle made nearly half a circle. The next experiment was on two small

needles in a spiral connected with the galvanometer. The galvanometer was powerfully affected, as in the first instance, but the needles were not sensibly magnetised: the trial was twice or thrice made with the same results. Next, a shock was received. Mr. Owen, to whose kind offices I was in part indebted for the permission I had obtained, had prepared me from the mention of his own experience, to expect that this would not be contemptible: it rather exceeded than fell short of my expectations: the sensation was rather like that imparted by a Leyden vial than the voltaic battery; more acute, less benumbing than that of the torpedo; and it reached higher, even beyond the elbows.* I did not extend the trials further, nor attempt to test it as a chemical agent, having been informed that Mr. Faraday, who preceded me in experimenting on this fish, had carefully investigated its influence in relation to this point.

Since that time this gentleman has communicated the results which he obtained to the Royal Society, from the Proceedings of which it would

* At the obliging suggestions of Mr. Bradley, by whose directions every assistance I could wish was afforded me, the insulated wires had attached to them bent pieces of sheet-copper, in the form of a saddle, for better contact with the fish. These were used in the two first experiments, and also in a fourth, when I had the pleasure of witnessing the production of sparks,—according to the method by which M. Matteucci, in 1836, succeeded when experimenting on the torpedo, and Mr. Faraday recently on this fish; in the third trial, the hands were applied immediately to the surface of the fish.

appear that, besides the spark, he has witnessed all the effects known to have been produced by the electricity of the torpedo.

Reasoning from my own comparative results on the electricity of the torpedo and gymnotus, it appears to me difficult to avoid the conclusion, that the power of each, whatever it may be in essence, is modified in its manifestations, or effects; the difference between the electricities of the two fishes in imparting shocks, and in magnetising steel, was too strongly marked to be any wise doubtful.

5.—*On the Fœtal Development of the Torpedo.*

The accounts we possess by different naturalists, of the mode of generation of the torpedo, are very discordant and perplexing, and consequently it is a fit subject for further research.

It may be advisable to premise a few particulars respecting its generative organs. The female, like those rays and squali which are considered oviparous, has two ovaria, a common oviduct, and two uterine cavities.* The ovaria, one on each side of the spine, are attached to, and enveloped in, a fold of the peritonæum, just above the liver, and a very little below the common infundibulum or opening of the oviduct. The oviduct passes round, on each side under the liver, and ends in an enlargement,

* Vide Plate II.

one over each kidney, which from its function may be called a uterine cavity, formed, like the duct itself, of an inner membrane, and of a peritoneal outer coat, connected together by loose filamentous tissue, and opening into the lower part of the intestine or cloaca, by a common mouth, a little posterior to the minute papilla—the termination of the ureters. In the oviduct, just above its enlargement into the uterine cavity, there is only a slight trace of a glandular structure ; in which respect the torpedo seems equally to differ from the different species of squalus, and of ray—all those which I have examined of either genus being possessed of a large glandular body in the situation mentioned.

The male generative organs consist of two firm oval testes, occupying the same situation as the ovaria in the female, and not very different in appearance,—of vasa deferentia, without vesiculæ seminales, and of a papilla in the cloaca, the common termination of the seminal and urinary passages, near the verge of the intestine.

Like the squali and the rays in general, the male torpedo is provided with two appendages, one on each side of the anus, composed of articulated bones, of muscles, of cartilages, and a glandular structure.

The eggs of the torpedo, I have never found in the oviduct in their passage, only in the ovaria, or attached to the ovaria, or in the uterine cavities. When mature, and attached to the ovaria, they are covered with a vascular membrane, through which

they break to enter the infundibulum. In the uterine cavity they are destitute of white; they are covered, before the appearance of the embryo, with a most delicate membrane or pellicle, and consist entirely of yolk. The number of eggs varies very much with the size of the fish; and in the smallest pregnant fish that I have examined, I have never found less than four in the two cavities, and in the largest not more than seventeen. Their size, too, varies; their average weight is about 182 grains; the largest of eighteen eggs which I have weighed, taken from five different fish before the embryo appeared, was equal to 210 grains. Though without a distinct white, there is with them in the uterine cavity, common to all of them, a little fluid generally milky, more rarely glairy, and sometimes bloody, which affords crystals of common salt on evaporation, and a very little animal matter, composed chiefly of albumen.

In describing the fœtal development of the torpedo, I shall confine myself strictly to what I have actually observed. In the first stage in which I have witnessed the embryo, it appeared as represented in Plate III. fig. 1, about seven-tenths of an inch long, without fins, or electrical organs, or any distinct appearance of eyes, with very short external branchial filaments,* not yet conveying red blood, and

* These filaments, variable in length and appearance, are constant in containing each a blood vessel which makes the circuit of the filament.

with a red spot in the situation of the heart (probably the heart itself), communicating by red vessels in the umbilical cord with the vascular part of the egg.

In the next stage in which I have observed it, it appeared as in fig. 2—not quite an inch long, or a quarter of an inch wide; the ventral fins visible, and also the dorsal, and the inferior portion of the great pectoral fins; the branchial cartilages distinct and naked, the electrical organs not having yet appeared; the external branchial filaments longer than in the preceding, but still comparatively short; some of them tipped with red blood, others carrying it.

The next stage of advance I have seen, is represented by fig. 3. This embryo was about one inch and one-tenth long, and four-tenths of an inch wide where widest, and it weighed just five grains. Its electrical organs were beginning to appear. The external branchial filaments were about six-tenths of an inch long, and contained red blood. The heart was distinct and large, as were also the two lobes of the liver. The stomach was small, apparently empty, smaller than the intestine. The intestine was large and white. The vitello-intestinal canal was distinct; it appeared as a very slender thread, connected with the upper part of the intestine, and like the intestine itself it contained no yolk. The eyes were apparent. There was a vesicle in the head distended with a colourless fluid, and the cavity of the cranium

was full of a similar fluid. The roots of the electrical nerves were visible, but no brain.

The next stage in which I have observed the embryo is represented by fig. 4. It was advanced only a little beyond the preceding ; the principal differences were in the electrical organs being a little longer, the branchial filaments considerably longer (about an inch long), and the brain and spinal cord apparent.

In the next stage in which I have seen it, as represented by figure 5, there was a very considerable advance. The fœtus was about two inches and a half long, and one inch and three quarters wide. The electrical organs were distinct, the pectoral fins entire, the external branchial filaments very long. The stomach was still small and empty, whilst the intestine was distended with yolk. The external yolk was covered with a vascular membrane, and not partially as in the preceding, but entirely. The vitello-intestinal canal freely communicated with the intestine, and was yet very little enlarged where it joins itself to the intestine at the commencement of its vascular portion.

The next stages, which have come under my observation are represented by fig. 1 and 2, Plate IV. and fig. 1, Plate V. The cavity of the abdomen is shewn laid open in the two former, to exhibit the external yolk in progress of diminution, and the internal yolk contained in a membranous bag, as it were a lateral extension of the vitello-

intestinal canal, in progress of accumulation.* The branchial filaments have almost entirely disappeared. I shall notice only two stages more of the young torpedo; the one of a fish six weeks old, (fig. 2, Plate V.) in which the internal yolk was considerably diminished in bulk; its connexion with the umbilicus almost absorbed; the intestine full of yolk; the stomach empty, but considerably developed: the other of a fish six months old, (fig. 1, Plate VI.) in which only a very small portion of the internal yolk remained, and the connexion of the inner yolk bag with the umbilicus was absorbed, a vestige only of the canal of communication remaining.

These fish, at about the full period of utero-gestation, were extracted from a torpedo just after she had been caught, were instantly put into salt water, and were preserved alive. I shall have occasion to revert to them further on.

* At this period of fœtal development the yolk has two distinct membranes, an external transparent one, and an internal vascular one. The former is of great delicacy generally, excepting where the egg joins the abdomen; there it is very thick and strong, and slightly opaque, serving in a manner the part of the sheath of the umbilical cord of the mammalia; it is connected with, and appears to end in the cutis of the abdomen. The latter, the membrana umbilicalis, or chorion, if it may be so called from its great vascularity, passes into the cavity of the abdomen, and terminates in the vitello-intestinal canal, from whence the internal yolk-vesicle proceeds. Two large vessels (the trunks of the vessels of the chorion) enter the cord-like termination of the egg. One of them terminates in the vena-portæ; the termination of the other I have not ascertained in a satisfactory manner; I believe it corresponds, in function, to the umbilical arteries, and brings blood from the fœtus to the egg, the other vessel returning it.

I may remark generally that I have never found in any of the gravid torpedos which I have examined in different stages, any membrane investing the fœtus, as is the case with the fœtus of some of the squali.* Neither have I found any fluid in the uterine cavity, at any period excepting that already mentioned.†

* The fœtus of squalus acanthias at a very early period is contained in a delicate membrane, which at a more advanced period, near the full time, disappears. The fœtus of the squalus squatina seems to be analogous to that of the torpedo, without a membrane; that of the squalus galeus has a membrane even in its advanced stage, appearing to be, as it were, a link between the torpedo and the oviparous rays, whose eggs, enclosed in a thick strong horny shell, (mus marinus, pulvinar marinum of the older naturalists) are hatched out of the body.

† I have in vain sought in the uterine cavity of the torpedo for lithic acid, which is so abundantly secreted by the kidneys of the chick *in ovo;* nor have I succeeded in detecting urea in the fluid it contains, a substance which I have found in a notable quantity in the fluid of the uterine cavity of the squalus squatina, and in abundance in the liquor amnii of the dog about the fifth week of pregnancy; and have also detected in the human liquor amnii at the full period. In the cloaca of very young torpedos, I have sometimes seen a transparent fluid, probably urine, but in too small quantities for examination. The nature of the urinary secretion of the adult torpedo, I have not yet been able to ascertain. I suspect that it is liquid, and that it is voided almost as rapidly as it is secreted—the fish being without a urinary bladder, and its cloaca of narrow dimensions.

Hitherto I have had few opportunities of examining the secretion of the kidneys of fishes, a subject which, I believe, has not yet been at all investigated, although in many respects interesting. Referring to my notes, I find, that in the instance of a fish, belonging to Cuvier's Third Order, "les Plectognathes," occasionally caught off the south-west coast of Ceylon, I detected, in 1819,

The facts I have stated relative to the development of the fœtus of the torpedo, though amply sufficient to demonstrate that this fish is not oviparous, are not incompatible with its being ovi-viviparous, as it is considered by the naturalists who have paid most attention to the subject. Yet I believe it is not strictly so, and that it is more correct to say that it is intermediate between ovi-viviparous, and viviparous; the fœtus, as I believe, deriving its support in part from the ovum, and in part from the parent. The principal fact on which I found this belief is, that the mature fœtus is very much heavier than the egg. In the three following tables, I shall give the statical results, substantiating this fact.

The first table will relate to the ovum just after it has entered the uterine cavity, or before the appearance of the embryo; the second to the ovum after the fœtal development has commenced, but has made little progress; and the third to the fœtus when mature or nearly mature, indicated by the total disappearance of the external yolk, or its being reduced externally to a very small bulk. In noticing the kind of torpedo, I shall use the popular names

lithic acid in its urine. The kidneys of this fish were large, and much lobulated; it had an oval urinary bladder, in which the *ureters terminated,* and the bladder itself communicated with the anus by a membranous *urethra,* the mouth of which was covered with a valvular membrane. The urine was liquid; the lithic acid in minute quantity, and was discovered after evaporation by the test of nitric acid.

by which they are designated at Rome, reserving for another place the consideration of its species. The exact time when the fish was caught will be given with a view to endeavour to determine its breeding season and period of utero-gestation.

Table I.

Kind.	When caught.	Number of Eggs.	Weight of each Egg tried.
Tremola . . .	March 30	8	200grains.
		5	200
Tremola . . .	May 21		185
			185
			198
Tremola . . .	May 24		188
		9	188
			193
			193
			193
			200
			210
Tremola . . .	April 29		167
		5	167
Tremola . . .	May 31	5	129
			140
			170
			165

Table II.

Kind.	When caught.	Number of Eggs.	Weight of each Egg tried.	Weight of each embryo attached, tried.
Tremola .	June 13	13	166 grains.	2 grains.
			140	2
			101	2
			156	2
			134	
			111	
			147	2
			174	3
			131	
			164	
			102	
Tremola .	June 26	14	79	12
			79	14
			115	12
			107	13
			108	11
			77	13
Tremola .	June 28	9	215	5
Ochiatella	June 29	4	120	
			119	
			114	

Table III.

Kind.	When caught.	Number of Fœtal Fish		Weight of each tried.	
		Male.	Female.	Male.	Female.
Tremola .	Sept. 6	2	3	540 grs.	580 grs.
Ochiatella .	Sept. 12	2	4	503	505
Tremola .	Sept. 15	1	4	435	457
Tremola .	Sept. 29	6	7	481	514
				487	495
				464	533
				452	506
				485	521
				428	519
					500

The mean of the results contained in these tables is, that the weight of the egg, before any appearance of the embryo, is 182 grains; and after its appearance, including the weight of the embryo, about 177 grains;—whilst the weight of the mature fœtal fish is about 479 grains, proving an augmentation of weight in the mature fœtus more than double that of the egg; and in this respect differing remarkably from the fœtal chick, which at its full time weighs considerably less than the original yolk and white from which it is formed, owing in part to the evaporation of water through the shell, and in part to the excretions going on, especially of lithic acid derived from the kidneys. How is this augmentation of weight to be accounted for? Is there, as in the majority of the mammalia, any connexion between the fœtus of the torpedo and the parent, through the medium of a vascular or cellular structure? or, has the fœtal fish in utero the power of feeding by the mouth, and of taking food into the stomach? or, does the uterine cavity of the parent fish secrete or pour out a fluid, which is absorbed by and in part nourishes the fœtus?

The first and second query I must answer in the negative; nothing that I have observed indicates any connexion such as that supposed in the first query, between the parent and fœtus. I have carefully examined the gravid uterus under water, thinking it possible that the villi of the uterine cavity might inosculate with the branchial filaments; but I could

not detect the slightest union of them or even apposition. I have carefully examined too the stomach of the fœtus in its different stages, and I have always found it empty. Admitting then that the augmentation is effected by absorption (the only way apparently remaining to account for it) another question arises—How is the absorption accomplished? Is the whole surface of the fœtus an absorbing surface, as in the instance of some of the mammalia which are destitute of a placenta, and whose fœtus does not appear to be connected with the uterus, as that of the opossum and kangaroo? or are the branchial filaments the principal absorbing organs?

It appears not improbable that both the general surface and the filaments are concerned in the operation. The late Dr. Monro, who observed these filaments in the fœtus of the common skate, supposed that they perform the same function as the gills, and are a substitute for them, like the branchial appendices of the tadpole; and the same view has been taken by others, of analogous filaments belonging to the fœtus of most of the squali. This function they may perform in common with the surface, and at the same time they may convey nourishment and material for growth. If I may hazard a conjecture, I would suggest that the matter which may be absorbed by the surface may enter into the composition of the body generally; whilst that which may be absorbed by the branchial filaments may be chiefly employed in forming the electrical organs, and per-

haps the branchial and the adjoining mucous glands. I shall notice a few circumstances which appear to me favourable to this conjecture.

1. The branchial filaments are most numerous, and of greatest length, whilst the electrical organs are forming; appearing just before these organs begin to be developed, and being removed when they are tolerably complete. Now it seems more reasonable to suppose that this associated progress of the two, is in the relation of cause and effect, than to imagine that the filaments are solely designed as a substitute for the branchiæ; especially as the blood in the vessels of the yolk membrane seems to be as well adapted to receive the influence of any little air which may be contained in the fluid in the uterine cavity, as the blood circulating in the vessels of the filaments.

2. In none of the squali, the fœtus of which I have had an opportunity of examining at different periods, have I found the same elaborate apparatus of branchial filaments: they have been less numerous, and very much shorter. Does not this greater elaboration indicate that they are intended in the torpedo for a special purpose? and when we consider the nature of the electrical organs, abounding in fluid, as well as their peculiar office, does it not seem accordant that there should be such a peculiar provision as that in question for their formation?

3. In one instance I found a large fasciculus, as represented in fig. 2, Plate VII, unconnected with the

branchial apertures, attached to the head, anterior to the eyes, in the situation of the principal cluster of mucous glands in the adult fish between the anterior portions of the two electrical organs. May not this be considered an *instantia crucis*, both as shewing that the branchial filaments are not solely designed as a substitute for the gills, and rendering it highly probable that they are concerned, not only in the development of the electrical organs, but also of the mucous glands ?

It is not necessary to discuss the other two modes in which the fœtus of the torpedo is nourished, analagous to what is witnessed in the chick *in ovo ;* first, by means of vessels conveying blood, passing from the yolk membrane, and afterwards in addition by the direct passage of the substance of the yolk into the intestine of the fœtus, through the vitello-intestinal canal.

Whether the fœtus of those squali and rays which are considered ovi-viviparous, are only nourished in these two ways, or also in the additional manner of the fœtus of the torpedo, is a subject for inquiry. From what I have observed, I am rather disposed to think that they are nourished in the latter manner, though in a less degree, and without excepting even those which are contained in a closed membrane.

From the facts given in the preceding tables, and from others which I have observed, it may be inferred that the torpedo does not bear young more than once a year ; that the breeding season is the latter end of

autumn and the beginning of winter,* and that the period of utero-gestation is from nine to twelve months.†

I have alluded, some pages back, to the fœtal torpedo at its full term. Whilst I was in Malta, though I examined more than two hundred torpedos, I found five only in which the young were arrived at or near this stage. Of these, three were brought alive. I shall give some particulars, chiefly of their broods, as they may not be considered uninteresting in themselves, and as they may tend to illustrate the slow growth, and some of the peculiarities of this extraordinary fish.

The first live torpedo that I obtained in this state was an occhiatella on the 12th September. It was 14 inches long and 8½ wide, and after the extraction of the fœtal fish, it weighed 1lb. 3oz. It had been caught rather more than an hour, and was in a small bucket of salt water. I immediately set about pre-

* According to Aristotle, it brings forth in autumn. In my first paper, I supposed, erroneously, that the principal breeding season is the spring, from the circumstance that the fish at that time abound in ova of a large size.

† I say from nine to twelve months, because I suspect the time of utero-gestation is not precisely fixed, but that it varies with circumstances favourable, or unfavourable to bringing forth; thus I have had young torpedos brought me, caught in the sea, in which the internal yolk vesicle was large; and in one instance I found them in utero, with this vesicle greatly reduced in size, so as to suggest the idea which Aristotle adopted, that the young of the torpedo after birth return at will into the uterus,—an idea which cannot be held, on account of its anatomical impossibility.—(*Arist. Hist. Animal.* vi. 10.)

paring an apparatus to try its electricity, which occupied me about five minutes. But it was too late, the fish was then motionless. As soon as the apparatus was ready, I opened the cavity of the abdomen, hoping that, if gravid, as asserted by the fishermen, the young might still be alive. From each uterine cavity three fish were extracted; but they were all dead,—neither in air, nor in salt water, did they show any the slightest signs of irritability, though they had no appearance of being bruised, or in any way injured. The size of each was nearly the same; the only difference I could perceive was a little variation in the magnitude of the external yolk-bag; in two it had all but disappeared; it was smaller than a barley corn; in the other two it was a little larger, and in the two others, perhaps, a little larger still. The internal yolk was very large and about the same size in all. Their organization generally appeared to be complete, even to their teeth.

The next fish that I obtained near its full time was a tremola, on the 29th Sept. It had been caught an hour or two before, and was in a very languid state, having been put into a vessel containing only just sufficient water to cover it. It was tried on the multiplier, but it did not affect the needle. When moribund, the abdomen was opened, and I extracted with the hand, without experiencing any shock, from the two uterine cavities, twelve fœtal fish, and one which had been expelled before, and was alive and swimming about, made thirteen. They were

all nearly of the same size; and of all of them, the external yolk-bag had nearly disappeared, the portion remaining being less than a small pea. Most of them appeared inanimate; two or three only moved their tails very slightly and the margins of the pectoral fins. They were as soon as possible transferred to fresh sea-water. After about two minutes, one or two of them began to move their water-valves.* I was now called away, and rather more than four hours elapsed before I returned. On my return I found them all freely respiring and moving about actively. They imparted smart shocks to the fingers or finger pressing the upper surface, and another of the same hand the under surface of the electrical organ;—they distinctly affected the galvanometer, and feebly magnetised needles through the medium of a spiral. These trials were made at 2 P.M.; at 10 P.M. all the fish were alive and vivacious; an hour after I found them all dead.

The third fish I have to notice was a tremola, 17 inches long and 12¼ broad. When brought on the evening of the 6th Nov. in a vessel of salt-water soon after it had been caught, it was tolerably viva-

* I apply this term to the valves which are situated at the openings behind the eyes, the office of which appears to be to force water into the gullet to supply the branchiæ; which water, in regular respiration, passes out through the branchial apertures; but occasionally is discharged (the latter being closed) in considerable quantities, through the superior apertures. Bloch erroneously supposes, that these latter are the natural outlets. He says, "Ils servent à l'animal à rejetter l'eau qu'il avale, soit en prenant sa proie, soit celle qui entre par l'ouverture des oüies."—*Hist. Nat. des Poissons*, iij. 667.

cious; yet it did not affect the galvanometer. Before it was quite dead, the abdomen was opened and the fœtal fish were extracted. They were ten in number, all of them about the same size; in all of them, the outer yolk-bag had disappeared as represented in Plate VI. Touching them with the hand in the act of removing them from the uterine cavity, I received a distinct shock, sharp though not strong. Put into fresh sea-water as soon as they were extracted, some of them immediately, and in a few seconds all of them, were active and swam about; and making trial of one of them instantly (the apparatus being in readiness) it powerfully affected the galvanometer and made a needle slightly magnetic. To ascertain the state of the internal yolk-vesicle, one of these fish was killed, by putting it into fresh water, about half an hour after its extraction. It immediately became very restless and endeavoured to escape; then in less than a minute it became quiet, and its water-valves ceased to act; two or three times at intervals it was again restless; in about twenty minutes it was motionless and dead. The appearance of its internal yolk-vesicle is represented in fig. 1. Plate VII.

Three of these fish remained alive till 22d May in sea-water, which was changed daily or every second day; of the others, one only died a natural death, the rest were killed at intervals, for the purpose of examining the size of the internal yolk-vesicle, which very slowly diminished during this

period, and as well as I could judge, in a very regular manner, supposing, when first extracted, that in all the internal yolk was nearly of the same size. Fig. 1. Plate VI. shews the diminution it had undergone on 22d May, when the three residual fish died, apparently from the carelessness of a servant, giving them turbid salt-water and weaker in salt than they had been accustomed to.

During the whole of this period of five months and more, they ate nothing, though very small fish both dead and alive were put into the water. They retained, and indeed increased in, activity, and even in their electrical energies, of which I made occasional trials. They also became of rather firmer consistence, and of a darker colour, and perhaps contracted a little in dimensions. The weighing of those first killed was neglected; of the three which died last, two (males) weighed 510grs. each; the other (a female) 560grs. Their stomachs were pretty largely developed, but empty; in the intestine there was a small quantity of yolk remaining, coloured greenish-yellow in the inferior part from the admixture of bile.* All these facts seem to shew a very slow development, and are in accordance with a long period of utero-gestation;—and I may add in favour of the same, that the ova in the ovaria

* I have never found the stomach of the fœtal fish, or of those fish which were so long without eating, softened or corroded,—a change which I have several times observed in the stomach of the adult fish, killed when there was food in it in process of digestion.

of all the three parent fish were very small, the largest of them not exceeding a pea, and the majority of them minute vesicles containing a transparent fluid.

Other inferences might be drawn from the details, especially in favour of the branchial filaments being absorbent organs, rather than supplying the place of gills, (the gills being apparently useless in utero when formed.)

In the beginning of this section, I have alluded to the discrepancy which exists amongst writers on natural history relative to the mode of generation of the torpedo. Aristotle always describes this fish as viviparous; so does Lorenzini, who wrote in the middle of the seventeenth century. On the contrary, Blumenbach, generally an accurate writer, though he quotes Lorenzini, gives the torpedo as an example of the oviparous cartilaginous fish, laying a few large eggs, protected by a horny shell; and, even Cuvier appears to have fallen into the same error, at least in his Règne Animal, he has not corrected it; and from his general account of the generation of the rays, both in this work and in the Histoire Nat. des Poissons, it is to be inferred. It is most probable that analogy and a want of confidence in Aristotle and Lorenzini, were the cause of this mistake; no doubt, had these able men enjoyed an opportunity of investigating the subject themselves, they would not have failed in ascertaining the truth. Even in Malta, the inquiry

is of considerable difficulty, requiring much time and patient waiting, owing to the great rareness of the gravid fish. Some idea of this may be formed, when I mention, that after I had begun the pursuit, more than twelve months elapsed, before I could procure a fish with young, though I examined a very large number in hope of finding one, and though I offered to pay the fishermen above fifteen times the market price of the fish.

6.—*On the Species of Torpedo in the Mediterranean.*

Respecting the number of species of torpedo found in the Mediterranean, naturalists have been much divided in opinion; some, as Rondelet, followed by Risso, admitting five species; some as Bellon, and latterly Rudolphi, limiting them to two; and others, as Linneus and Bloch, with Willoughby, Ray, and Artedi, admitting only one.*

That there are two distinct species in the Mediterranean, namely, the occhiatella, and the tremola, as the two kinds are vulgarly called at Rome, the

* Both those who have adopted four species and those who have allowed only one, appear to have followed Rondelet—in the latter instance critically, in the former literally; in proof of which the following passage may be adduced. "Torpedinum genera quatuor facimus tria earum quæ maculis notatæ sunt, quartum quæ maculis caret. Quæ genera omnia viribus et corporis specie non differunt, sed maculis tantum. Quare quæ de unius facultatibus et partium tum internarum, tum externarum descriptione dicuntur, eadem etiam reliquis convenire existimatio."—G. Rondeletii, Libri de Piscibus, &c. p. 362. fol. Lugduni, 1554.

spotted and non-spotted of Bellon, there does not appear to me to be a doubt. But it appears more than doubtful, if any other true species exist in this sea.* I draw this conclusion from multiplied observations made both at Rome and in Malta. That these fish, the occhiatella and tremola, are distinct species, admits of satisfactory proof.† They differ not only in their colour and general appearance, but also somewhat in their form. The occhiatella is more gracefully made than the tremola; its fins are larger, especially its dorsal fins; its water-valves are larger and different in shape; and the openings behind the eyes to which they belong, are guarded by much smaller projections than protect those passages in the tremola. And internally there is a remarkable difference in the structure of the villous coat of the uterine cavity; in the occhiatella, the villi are filamentous and detached, as represented in fig. 1, Plate II.; in the tremola they are continuous

* Probably in other seas there are other species of this fish. Dr. Andrew Smith informs me, that at the Cape of Good Hope there are two species, perfectly distinct from those of the Mediterranean; one of them indeed belonging to a different subgenus, well marked. It is his intention to describe them, in his interesting and able work, now in progress of publication, on the Zoology of Southern Africa.

† Cuvier, in the edition of his Règne Animal of 1830, distinguishes the occhiatella by its spots, and by the absence "de dentelures charnues au bord de ses évents." This does not hold good of any of the species which I have examined. The cartilaginous projections (which they really are) covered with cutis, I have found only smaller in the occhiatella, not absent.

delicate plates or laminæ, as shewn in fig. 2.* These characters are constant in all the different specimens which I have examined.

To these two well-characterized species, it appears to me, that all the varieties of the torpedo, at least those known hitherto in the Mediterranean may be referred: the T. *unimaculata* of Risso, and the second species of Rondelet,† to the Occhiatella; and the varieties, with dark irregular spots, or without spots, to the Tremola.

Cuvier in the last edition of his Règne Animal, and Rudolphi, have so considered the first-mentioned variety, the T. *unimaculata*, as it differs only in having one eye-spot, instead of five, the most common number. But, it is not more uncommon to meet with it, having three or four spots than one; and that this is purely accidental, is proved by the circumstance that in a brood of several fœtal fish, of which all but one resembled the parent in having five spots, the exception had three. The occhiatella has been seen even with seven eye-spots.

The varieties of the tremola, are the T. *mar-*

* The villi increase in size during the period of pregnancy, and then contain a large quantity of blood. In each filament in the instance of the uterus of the occhiatella there is a blood-vessel reflected on itself, circulating blood of a bright scarlet hue; in the lamellar structure of the uterus of the tremola there is a similar appearance of blood vessels in loops.

† "Secunda Torpedinis species à prima differt, quòd maculas, nigras, rotundas, circulis non distinctas habeat, sed eadem pentagoni figura dispositas. Est etiam primæ concolor."—*Rondelet, p.* 362.

morata of Risso, and the T. *galvanii*, which Rudolphi, and I believe latterly Cuvier, has considered identical in species. This appears to me to be proved by their general character being the same, their water-valves, fins, and interior structure; and further, by the circumstance that between the two varieties (the former marked with black spots or patches irregularly distributed, the latter without spots) there is a complete gradation or intermixture, both the spotting and the colouring varying infinitely, so that it is difficult to find two fish exactly similar in either respect. How much this variation is owing to locality and other circumstances, it is difficult to decide. As the spotted fish is most frequently caught where the bottom is sandy, and the other variety where it is muddy, light may be concerned in the difference, and the spots may be produced like freckles, by the action of light; and in process of time, they may become hereditary, the fœtuses generally, even of those varieties, resembling in appearance the parent fish.

Rudolphi has given to the occhiatella species the Italian name of T. *ocellata;* perhaps the Latin word *oculata** may be preferable. The other species he designates as T. *marmorata*, for which might

* Pliny, by Bloch and others, is supposed to have applied this term to the torpedo. As in the only passage in which I am aware he has used it, (Hist. Nat. xxxii. c. ii.—the passage is little more than a list of fish) the word Torpedo is also employed, as if applied to a different fish, the justness of their supposition is doubtful.

be substituted the term *diversicolor*, being applicable to all the varieties of it, and descriptive of its quality of variableness of appearance.

For the information of travellers who may visit Malta, and wish to investigate the electricity of the torpedo, I may mention that this fish (both the T. *oculata*, and *diversicolor*) is called, by the Maltese, Haddayla, derived from a verb in the language, signifying to benumb or paralyze; and con sequently that it should be inquired for by this name, not by that of Torpedo, which is generally unknown there. I may add further, by way of caution, that the torpedo in Malta, is often difficult to be procured, partly owing to its being little sought after for the table, being used as an article of food only by the indigent; and partly, I believe, from the uncertainty and irregularity of its coming into shallow water. However, by paying well the fishermen, it may be obtained at all seasons, and the longest time to wait may be a fortnight or three weeks.

7.—*Miscellaneous Observations on the Structure of the Torpedo.*

In this place, I propose to give an account of such observations as I have incidentally made on the structure of this fish, with the wish that they may be received as a contribution towards its natural history. The torpedo has often been brought forward as a type of the order of cartilaginous fishes; this, and

its extraordinary electrical function, impart to it peculiar interest, and seem specially to recommend it as an object of various research.

The form of the skeleton of the torpedo is well exhibited in Plates VIII. and IX. The drawings were very accurately made from a skeleton of a Torpedo oculata, a male, about six inches long, carefully prepared.

The bones of which it is formed, as far as I have examined them, appear to be all similar in composition. Although apparently entirely cartilaginous, they are not so in reality; they contain earthy matter in the form of phosphate and carbonate of lime in variable proportions; the cartilage, probably, is the original basis, the earthy matter a superadded deposit. Viewed under the microscope, this compound nature is very manifest. Take, for instance, the principal bone of the pectoral fin; a very thin slice of it—a transverse section appears as if composed externally of irregular polyhedrons, themselves consisting of minute plates and fibres, transmitting little light, reflecting more, and perhaps crystalline; whilst internally it is nearly transparent, and nearly homogeneous, even when dried, exhibiting only a few opaque points. The portion thus examined was previously stripped of its delicate fibrous enveloping tissue, its perichondrium.

That the more opaque and obscurely crystalline part owes the peculiarity of its appearance to the presence of phosphate and carbonate of lime, is proved by the action of an acid. Immersed in dilute

muriatic acid, there was a slight disengagement of gas, and after a time the opaque matter was removed, and the acid, tested by ammonia, &c., was found to contain in solution, lime and phosphate of lime.

Different parts of the skeleton vary in firmness, and degree of hardness, and also of elasticity, according, I believe, to the proportions of the principal constituent parts, the cartilage, water, and earthy matter. In those bones which are most flexible, soft, and transparent, as the fin-rays, there is very little earthy matter, and the cartilage contains a large proportion of water. In those, on the contrary, which are most dense, as parts of the head, especially that part corresponding to the os petrosum in mammalia, and the vertebræ, the quantity of earthy matter is comparatively large, and the cartilage is of a compacter kind, in which there is a smaller proportion of water.

The flexibility of the majority of animal textures is intimately connected with the presence of water. The skeleton of the torpedo, and of other fishes of the same order, offer a striking illustration of the fact: when the water is expelled by exposure for a sufficient time to a temperature of 212°, the most flexible parts become quite brittle: and by absorption of water, they again become flexible.

I have mentioned that cartilage may be considered as the basis of the skeleton; independent of other considerations, especially connected with fœtal development and growth, this may be inferred from the circumstance that, though the forms of the different

parts are not in the least altered by the action of an acid which removes the earthy ingredients, they are, with the exception of the vertebræ, entirely destroyed by calcination, and I believe the putrefactive process has a similar effect.*

The only part of the skeleton which I have analysed in its moist state, was a portion of the principal bone of the pectoral fin, belonging to a fish (T. oculata) about eight inches long. It was found to consist of

19·3	cartilage.
67·0	water.
10·7	phosphate of lime.
3·0	carbonate of lime and saline matter, principally common salt.
100·0	

Or, without water, of

58·2	cartilage.
41·8	earthy matter, &c.

On the dried skeleton, I have made a few more experiments. For the purpose of comparison, the

* This may help to account for the vertebræ of cartilaginous fishes, as well as their teeth, being of common occurrence in the fossil state; these retaining their form, when their animal portion is decomposed; the other parts of the skeleton losing their form, being reduced to dust by the change. On the same principle, the bony plates constituting the outer integument of the squali, and the spines and bony tubercles connected with the skin of many of the rays, may be expected to be found in the fossil state, as from the experiments which I have made on them, it appears that their form is not destroyed by the action of fire.

whole was divided into three portions—the cranium, the spine, and the remaining parts. The cranium was found to consist of

57·5	cartilage.
33·4	phosphate of lime.
9·1	carbonate of lime and saline matter.
100·0	

The spine, of

51	cartilage.
49	phosphate and carbonate of lime, &c
100	

The other parts of the skeleton, mixed together, of—

63·3	cartilage.
32·7	phosphate of lime.
4·0	carbonate of lime and saline matter.
100·0	

The teeth of the torpedo are comparatively very small, so much so, that to be seen distinctly, the aid of a magnifying glass is necessary. There are five or six rows of them, attached to each jaw, or rather to each lip, just within the mouth, inserted in the lining membrane. They are irregularly conical, slightly arched, and inclining backwards, and termi-

nate in a point. Slightly moveable, they offer no resistance to objects entering, they yield and allow them to pass into the gullet; but not so in the contrary direction; pressed on from behind, they are raised so as to be nearly, not quite erect; acting as hooks they effectually prevent their prey from passing out.

The teeth are very numerous; I estimated the number of those attached to one lip at 180. So small were they, that including the integument in which they were fixed, and that in its moist state, they weighed only half a grain.

Their form is best brought into view by the action of fire. Before the blowpipe, the membrane to which they are attached burns with flame, and the teeth remain unchanged in shape.* They are now seen to consist of a broad irregular basis, by which they were fixed, and from which the projecting conical exposed part arises. Like scale-armour the bases are so arranged as to be flexible only inwards. Chemically examined, I have found them to consist of phosphate of lime, and a little carbonate of lime and animal matter: the proportions of each I have not ascertained. Their form is not altered either by the solvent power of an acid exercised on the earthy ingredients, or by the decomposing effect of fire acting on the animal matter.†

* If the heat of the blow-pipe is long continued, the teeth fall to powder.

† Analogy would lead to the inference, that the composition of

The mucous system of the torpedo, which I have already briefly noticed, is well deserving of attention,

the skeleton of other fishes of the same great order is similar. I have hitherto only examined a few specimens: the results were in perfect accordance with the analogy.

All the fishes of this order are peculiarly endowed with means of defence, and in a large number of instances with those of offence, both curiously various. Thus the squali with powerful teeth and fins, well adapted to their fierce and predatory habits, have crustaceous integuments of stony hardness, and great toughness, possessing much power of resistance, and consequently well adapted for defence; the outer osseous layer of their skin is, in fact, a natural scale-armour. Take for instance the Ginglymostoma Gata, (Müller) the integuments of which I have carefully examined; the natural armour alluded to, is composed of anvil-like pieces, like the teeth, firmly impacted in the cutis, yet admitting of motion. A specimen of the entire integument, I found, in its dry state, to consist of

29·3	Phosphate and corbonate of lime, with some saline matter.
70·7	Animal matter.
100·0	

The rays, with comparatively feeble fins and teeth, ill fitted for attack, are admirably provided with defensive means; such as the torpedo, with its electricity, the other species, with spines variously distributed. These spines appear to be analagous to the osseous cuticle of the squali. The only ones which I have examined are those of the common thornback. I have found the bony tubercle from which the spine proceeds, composed of

36·2	Phosphate and carbonate of lime, with some saline matter.
21·4	Animal matter.
42·4	Water.
100·0	

and of particular study, especially that of the integuments, in which the mucous organs are very strongly developed.

As before mentioned, the cutaneous mucous system consists of two parts,—of clustered glandules by which the mucus probably is chiefly secreted, and of excretory ducts or tubes by which it is conveyed to and distributed over the surface. The two figures in Plates X. and XI. display the glandular and tubular structure: one is a drawing of a small torpedo oculata, the integuments partially dissected and reflected, one electrical organ removed, and a principal cluster of glandules and their ducts thereby brought into view;—the other is from a carefully made drawing of a fish of the same size and kind in its natural state, shewing the mucous tubes distributed under

And the spine itself of

65·5	Phosphate of lime and carbonate, with some saline matter.
24·2	Animal matter.
10·3	Water.
100·0	

Whilst the cartilage of the great pectoral fin consisted of

3·7	Phosphate and carbonate of lime, with some saline matter.
19·1	Animal matter.
72·2	Water.
100·0	

They were taken fresh from the fish and wiped apparently dry, before subjected to experiment. The spine and its tubercle retained their form after the action of fire; the cartilage lost its form.

the surface with their external apertures just as they appeared in the fresh fish, in which they were visible in an unusually clear manner, even for a young torpedo.*

The clusters of glandules, as far as I have observed, are never less than four in number. These are pretty regular in their situation; the largest is to be found, on dissection, in the mesian line, between the anterior extremities of the electrical organs, surrounded by a soft gelatinous substance. A smaller cluster occurs just anterior to each eye, similarly imbedded. These are supplied with nerves from the fifth pair. Two other clusters occur, one on each side of the electrical organs, between them and the pectoral fins, as represented in the Figure. I have occasionally found another pair of these clustered glandules, nearly in a transverse line with the preceding, between the margin of the electrical organs and the gills. These latter are supplied with nerves from the sixth pair. In addition to these principal clusters, there are very many much smaller clusters and solitary glandules scattered under the skin, in the cellular tissue, furnished with short excretory ducts: they are most distinct, in the upper margin of the body, between its extreme circumference and the border of the electrical organs.

The excretory ducts which proceed from the

* On account of a greater degree of transparency of the skin of the young fish, it is very much better adapted than an old or full grown one for the examination of the mucous system.

glandules are all of comparatively large dimensions. They vary, however, in size; there are at least three or four different orders of them. The largest are longest, and convey the secretion furthest from its source. They are all transparent; they contract very much when immersed in boiling water, and do not immediately become gelatinous like the skin. They have generally each a simple minute orifice: I have occasionally found them having two. The mucus which they convey is in appearance not unlike the matter of the vitreous humour of the eye. It dissolves completely and rapidly in boiling water.

The alimentary canal of the torpedo may be divided into six parts:—1st, the mouth and pharynx, in which there is no tongue, even rudimentary; the latter perforated by the two temporal apertures:—2dly, the œsophagus, lined with a distinct epithelium, longitudinally plicated and communicating with the branchiæ laterally by five apertures on each side:—3dly, the stomach, thickly membranous, and irregularly rugose; without any apparent epithelium, or any visible villi:—4thly, the first division of intestine, corresponding to the duodenum, short, irregularly valvular, and villous, communicating with the liver and pancreas by their respective ducts:—5thly, the second division of intestine, regularly and amply valvular, the valvulæ villous;* and lastly

* The valvulæ are composed of eleven transverse plates, with a central aperture in each, without which every pair but the first and last would form a closed cell. The first and last plate correspond only

the third, and inferior division, without valvulæ, destitute of apparent villi, corresponding to the large intestine of the mammalia, and birds, communicating near its termination with the urinary and sexual organs by their respective canals, and also with a small hollow nearly heart-shaped glandular appendicula, attached to its upper part, within the folds of the peritoneum, the function of which I believe is to secrete mucus.

The diagram, Plate XII. fig. 1. shews the form and dimensions of the stomach and intestines, and of the pancreas and spleen in situ, taken from a fish about 18 inches long. The stomach is the only part of the canal which is arched.* Its long pyloric portion is striated; its great arch is most distinctly rugose.

The alimentary canal, although not so amply supplied with mucous glands as the integuments, which in the majority of fishes may be considered as the principal mucous membrane, is not deficient in these organs: they are largest and most distinct in the œsophagus; smaller and more numerous in the stomach, and still smaller and comparatively apparently few in number in the intestine. They

to small sections of the others. Some of the plates are connected together by slender oblique processes.

* Lorenzini, in his account of the torpedo, denies its having an œsophagus:—"Lo stomaco è continuato con la bocca senza esofago, formando con essa bocca una sola et una medesima cavità, la quale a proporzione dell' animale è vasta,"—and he gives a figure in accordance with his description.

are situated in the submucous cellular tissue, and are without visible excretory ducts.

The liver is large, composed of two lobes (the right exceeding in size the left) connected by a long narrow isthmus of the same structure. It abounds in oil, and in consequence its specific gravity is less than that of water. Every other part of the fish is of greater specific gravity than water; and no other part of it contains any fatty or oily matter. In this respect I believe all the other cartilaginous fishes, and the majority of the flat fishes, are analogous; a large number of them abounding in mucus; all destitute of fat. The liver secretes a transparent, limpid, clear bile of a fine green hue, very bitter, and which, as it is not in the slightest degree viscid, even after lodgment in the gall-bladder, probably contains no mucus. The gall-bladder is large and globular; it is connected with the intestine by a long common duct, and with the liver by two hepatic ducts, one only from each lobe. Although the gall-bladder is half imbedded in the substance of the liver, I have not been able to detect any direct communication between them. The common duct, in approaching the intestine, enlarges, and it at the same time becomes thicker, but whether owing to the addition of muscular fibres, or of a mucous glandular structure, I do not know. Where it passes between the coats of the intestine, it is very much smaller; its bore there is extremely contracted. It ter-

minates, after penetrating the intestine, in a very fine short papilla.

The spleen is comparatively large and firm; the pancreas small: both in relative situation and structure, these organs very much resemble those of the mammalia. The kidneys are of moderate size, extremely lobulated, and resemble very much those of reptiles. They communicate each by a single delicate ureter with the extremity of the intestine, into which they enter separately, very little apart from each other, a little posterior to the entrance of the vasa deferentia in the male, and of the uterine cavities of the female. Their secretion is, it may be inferred, liquid urine, and it probably is voided almost as rapidly as it is secreted. I have in vain attempted to collect it for examination.

The heart of the torpedo is large for the size of the fish. Its single ventricle, communicating with the pulmonary artery, is thick, red, and very muscular. The trunk of the pulmonary artery rising from it, is provided with nine valves apparently muscular, arranged in three contiguous rows. Above the valves it gives off laterally, at right angles, three branches on each side, and terminates in a central main branch, which soon divides, and again redivides, to proceed to the gills. The ventricle communicates with a single auricle by a passage furnished with six valves, attached by delicate tendinous cords. The thin auricle, capable of much

distension, communicates with the two large veins from which it receives blood by a common passage guarded by two thin semilunar valves. The great veins pass to the auricle through the cartilaginous parieties of the sternum, where they may be considered as sinuses.

The brain of the torpedo, as in the cartilaginous fishes generally, does not fill the whole of the cavity of the cranium: the older and the larger the fish is, so much the greater is the space unoccupied by it. This space is commonly filled with a transparent fluid; I have never found in it any fatty or oleaginous matter.

Of the organs of sense, I shall only briefly notice those of sight and smell, which are both very elaborate in structure. The globe of the eye is moved and governed by four *recti* muscles, and one oblique muscle. The olfactory nerve is beautifully distributed on laminæ contained in a funnel-formed membrane, not unlike the sclerotic coat of the eye, and its external aperture is defended by delicate cartilaginous folds, covered with skin, somewhat resembling the external ear of the mammalia. And it is worthy of remark, further, that the cup or funnel-formed membrane, is itself contained in a bony cavity, having resemblance to the socket of the eye.

Under the head of the species of Torpedo and its varieties, mention has been made of the diversity of colour and of markings exhibited even by the same species. In two instances, in young fishes of the

spotted kind, (T. *oculata*,) I have observed another peculiarity,—filaments, like thick hairs or slender bristles, externally growing from the integuments; they are exhibited in Figure 2, Plate XII. attached to the margin of the temporal apertures, and also to the tail. From a slight examination which I made of them, they appeared to be very analogous to hair.

II.

ON THE URINARY ORGANS AND SECRETION OF SOME OF THE AMPHIBIA.

During a period of nearly four years that I was stationed in Ceylon, viz. from 1816 to 1820, I availed myself of the favourable opportunities which there offered to examine the urinary organs and urine of many of the animals comprehended in Linneus's class of Amphibia.

Previously, the subject had received but little attention; the organs had been very imperfectly described, and no account that has come to my knowledge, had then been published, of the chemical nature of their secretion.

The results of my observations were communicated to the Royal Society; they were honoured with a place in the Philosophical Transactions; and were mainly as follows.

1.—*Of the Urinary Organs and Urine of Serpents.*

The kidneys of several different kinds of serpents, which were submitted to examination, resembled

each other generally, although in each kind there were minute and trifling differences. In every instance, they were very large, nearly equal in size to the liver; they were long and narrow, and very lobulated; and like those of some of the mammalia, with conglomorate kidneys, they were destitute of a pelvis, each lobule sending a small duct to the ureter. Each ureter, formed by the union of two branches, terminated in a single papilla, situated in the cloaca, and a little elevated above its surface, the point of the papilla containing its orifice, directed towards a receptacle, into which the urine entered. The receptacle here alluded to is a continuation of the intestine, yet it may be considered distinct both from the rectum and cloaca, with which it communicated only by means of sphincter orifices. This conformation of parts was seen to advantage in the larger species of snakes; I first observed it in the python, and in a large coluber, commonly called the Rat-snake, frequently met with from eight to ten feet long.

The urinary ducts, even in the substance of the kidneys, were often observed to be of an opaque white colour, owing to a white matter which they contained, which was visible through their transparent coats, and which could be expressed from the papilla, and collected in small quantities for examination. More or less of a similar white matter was almost constantly found in the receptacle, generally in soft lumps, rarely in hard masses. In

the receptacle, I always observed it pure, and entirely free from fœcal matter. This solid urine, for such it was in reality, had gradually accumulated in the receptacle, until it formed the masses just described. From the observations made on snakes which were watched in confinement, it was to be inferred, that the masses were a considerable time in collecting, from three weeks, to a month or six weeks. When the bulk of the masses was so large as to distend the part unduly, they were expelled by an unusual exertion of the animal, most commonly in the act of devouring its food, which was usually taken periodically, at intervals of from three to six weeks. The urine, in the instances in which it was observed, was voided, accompanied occasionally but never mixed with fœces. When expelled, it was commonly in a soft state, of a butyraceous consistence, which it lost from exposure to the air, when it became hard and in appearance like chalk; a change which I believe was owing merely to the evaporation of moisture. The quantity of solid urine secreted by snakes, is very great, even more than might be expected from the size of their kidneys; I often saw masses, voided by large snakes, which weighed three or four ounces.

The chemical nature of this urine was such as I expected to find it: I say, expected, because, before I left England, I had been told by Dr. Prout, that he had examined the excrement of a serpent in London, and had ascertained that it was nearly

pure lithic acid. Such I found it in every instance, in at least eight in which I tried it; and its properties, whether fresh from the ureter, or after it had collected in the receptacle, or later when voided, were not materially different, excepting, as I believe (and this is matter of after-inference formed on reflection) that the fresh secretion contained no free ammonia, or even lithate of ammonia, the existence of which, I apprehend, when it occurs, is owing to partial decomposition. Subjected to examination before the blow-pipe, it emitted strong ammoniacal fumes, consumed without flame, and afforded only a very minute quantity of ash, which consisted chiefly of phosphate of lime, and a fixed alkaline phosphate, and a little carbonate of lime. In muriatic acid, it was insoluble. In warm dilute nitric acid, it dissolved with effervescence, and the solution evaporated afforded the pink residue, almost peculiar to lithic acid. In an alkaline ley, it was soluble, and the solution was precipitated by muriatic acid. These properties sufficiently proved that the nature of the urine was as above stated. Besides lithic acid, I was not able to detect any other ingredient, nor do I believe that the specimens which I examined contained any other, with the exception of a minute quantity of mucus with which they were lubricated and mixed.

2.—*Of the Urinary Organs and Urine of Lizards.*

Whilst in Ceylon, I examined the urinary organs of four different species of lizard; the gecko, iguana, a large species resembling the iguana, called by the natives kobbera-guion, and the alligator. The shape of the kidneys was found to vary in the different species; in their general structure, they appeared to be essentially the same. Each ureter terminated in a distinct papilla, and both papillæ were situated in the receptacle itself. In no other respect was I able to discover, between the urinary organs of these lizards and the snakes which I dissected, any material difference. Neither did the urinary secretion of these four species, and of many other species which I then and have since analysed, differ from that of snakes in its essential nature; in every instance it proved to be nearly pure lithic acid. Two specimens of urine, from different alligators, agreed in this circumstance; they differed, however, in one having no odour, the other a strong odour of musk; the former, was from a very young animal, the other was from an older one.

3.—*Of the Urinary Organs and Urine of the Turtle and Tortoise.*

The only species of Testudo, the urinary organs of which I have examined, are the mydas, geometrica, and græca; the two former, in Ceylon; the last,

recently in this country. Their kidneys, in their lobulated structure, resemble those of the preceding animals. The proportional size of the kidneys of snakes is greatest, that of the lizards next, and that of the turtle and tortoise least. As is well known, these latter animals differ from the former, in having a urinary bladder. The large green turtle of Ceylon has a pear-shaped urinary bladder, not unlike that of some of the mammalia, communicating with the cloaca by a long dilatable neck, or, as it may be considered, urethra, in which, nearer its termination in the intestine than its origin, the ureters open by two large prominent papillæ. The Testudo græca has a large bladder, much resembling that of the toad, of the like delicate structure, and divided into two compartments posteriorly; but, similar to the turtle, in its communication with the cloaca and the termination of the ureters, with this slight difference, however, that they are still more distant from the bladder, opening close to the extremity of the urethra.

In the bladder, both of the turtle and tortoise of Ceylon, I found flakes of pure lithic acid; but in no great abundance: they were suspended in a transparent watery fluid, containing a little mucus and common salt, but no urea, or any other substance, which I could detect in the small quantity the subject of experiment.

The urinary bladder of the Testudo græca was in a state of distension. Its contents were found to consist of lithic acid and lithate of ammonia, partly

in little firm masses, and partly in the form of sand and gravel, not crystalline, mixed with a little urea. The latter was in solution ; it was detected in a very minute quantity of brown liquid, the only portion of the urine that was fluid.

This tortoise, it may be remarked, was brought from the Mediterranean (I believe, from the Ionian Islands) in the summer of 1836. It passed the winter, according to its habit, in a torpid state. It was on the first of March following, that it was subjected to examination. Then its period of hybernation was just over ; it had begun to shew signs of activity. For several months it had ate nothing, and, it is believed, had not voided excrement of any kind. The temperature of the air of the room at the time was 55°, out of doors 50°, the thermometer placed in the rectum fell to 48°, in the blood flowing from the right auricle, it rose to 50°, and in that from the ventricle to 51°.5.

4.—*Of the Urinary Organs and Urine of the Frog and Toad.*

The examination which I made of the urinary organs of these animals in Ceylon, was confined to one species of each,—the bull-frog (Rana taurina, Cuv.) and the brown-toad (Bufo fuscus, Laurentini.) The former was procured from the lake of Colombo, where it occasionally grows to a great size;—the latter from the streets of the Pettah, which it frequents at night.

The kidneys of the bull-frog, on examination, were found to be apart, one on each side of the spine;—comparatively pretty large,—very much lobulated,—of a bright red colour and rather tender. The ureters did not terminate in the bladder, but in the rectum, by two soft papillæ, projecting a little, and situated between the orifice of the bladder and the anus,—nearer the former than the latter. The urinary bladder was of large dimensions, nearly globular,—semitransparent, and yet pretty strong and contractile, and evidently muscular. It opened into the rectum a few lines behind the anus, by a large orifice, very well adapted to receive the urine, as it flows from the ureters, when the anus is closed, as it usually is during the life of the animal, and soon after death, by its powerful sphincter muscle.

The urinary organs of the brown toad resembled, in most respects, those of the green frog. In two specimens, out of many that I dissected, I found the kidneys incorporated at their upper ends. The ureters had the same termination nearly. The bladder of urine appeared to be double; when distended fully with air, it resembled two oval bags; which, as two compartments, communicated freely just above the symphysis pubis, to which they were firmly attached; and they communicated also by a single orifice with the rectum. This orifice was as well suited as that of the former for the reception of the urine as it flows into the rectum. The urine of the bull-frog, taken from the bladder immediately after the death of the animal, varied a little in its appear-

ance in different instances, and very considerably in quantity, the bladder being sometimes full almost to distension, and at other times quite empty. The following is a description of a quantity of urine which amounted to 300 grains, and which was collected from thirty-six frogs of different sizes.

It was almost transparent and like water in appearance. It was insipid, but was not without smell; the odour from it was not unlike that of the serum of blood. It was of specific gravity 1003. It had no effect on litmus or turmeric paper. Slowly evaporated, it afforded a minute quantity of brownish extract, which had the smell of urea before it is purified. This deliquesced on exposure to the air. Decomposed by heat in a small glass tube, it yielded a little amber-coloured fluid and strong ammoniacal fumes, and a residual coal, in which was detected a large proportion of common salt and a little phosphate of lime.

Another specimen of the urine of these frogs was found to be more dilute; it contained a minute portion of common salt and of phosphate of lime, without any appreciable quantity of urea.

The urine of the brown toad was pretty uniform in its appearance in different instances; and in its composition also, judging from the results of the experiments made on it. From the bladders of eighty-four toads, 732 grains of urine were collected. Examined when quite fresh, it was nearly transparent, and would have been perfectly so, but for a

few minute flocculi suspended in it. It was of a pretty bright straw-yellow, very like healthy human urine in appearance, with the peculiar smell, and nearly the same taste in a slight degree. It was of specific gravity 1008. It did not alter litmus or turmeric paper. Nitrate of silver dropped into it produced a very copious precipitate of luna cornea. A solution of corrosive sublimate occasioned a minute flocculent precipitate; neutral acetate of lead, a copious white precipitate. *Aqua ammoniæ* had no effect; oxalate of ammonia produced a slight cloudiness; and a faint cloudiness was produced by muriate of barytes, which did not disappear on the addition of a drop of nitric acid. A portion of this urine slowly evaporated, afforded a brown extract, with a strong urinous smell. To the moiety of this extract of a syrupy consistence, a drop of nitric acid was added; the effect produced was just the same as if concentrated human urine had been the subject of the experiment; a crystalline compound immediately formed, which I could not hesitate in pronouncing nitrate of urea. The other moiety, decomposed by heat in a glass tube closed at one end, afforded a considerable quantity of yellow oily fluid, strongly impregnated with carbonate of ammonia, and a residual coal, from which was obtained a large proportion of common salt, and a little phosphate of lime, and slight traces of a fixed alkaline phosphate. Another portion of this urine was set aside to undergo spontaneous decomposition. After having been kept

eight days, it became slightly turbid and emitted a distinct, though not strong, ammoniacal odour.

The conclusions to be drawn from the results of these experiments hardly require to be pointed out, viz. that the urine of the bull-frog and of the brown toad contained urea as a constituent part, and the latter rather abundantly. And reasoning from analogy, it appears highly probable, that the urine of frogs and toads in general, is of a similar nature, and altogether different from that of the other amphibia.

It is seldom that any very abrupt transitions are to be observed in nature: the urinary organs of the turtle and tortoise, on the one hand, seem to be a connecting link between those of the reptiles and the mammalia, especially as regards their form of bladder and the termination of their ureters; and on the other, amongst the reptiles themselves, to form a gradation between the snakes and the batrachians.*

Perhaps, additional facts are not required to prove, that the nature of the secretion of the kidneys of animals depends more on the intimate and invisible structure of these organs, than on the kind of food the animals consume. Were such facts wanting, there would be no difficulty in furnishing them. How different is the urine of the brown toad and that of any species of small lizard! Yet flies are the

* The gradation is even more complete through the *Menobranchus lateralis* of the Canadian lakes: its bladder is very small: its ureters terminate in the cloaca, close to the entrance of the bladder.

favourite and common diet of both. Other remarkable instances might be mentioned of similarity of diet and difference of urinary secretion; and *vice versa*, instances might be afforded of difference of diet and similarity of urine: I will mention one only; it is that of parrots and snakes; their urine, as I have found, being much the same, consisting chiefly of lithic acid, though their diet is altogether different, the birds feeding entirely on vegetable matter,—the reptiles entirely on animal matter.* But let me not be supposed to maintain that the urinary secretion depends entirely on the organ, quite independent of the nature of the food, or of the blood, from which the elements of the urine are derived. It appears to be pretty satisfactorily proved, that, *cæteris paribus*, there is a certain relation between the nature of the food and of the urine. Whilst this has been generally admitted, the relation between the organ and the secretion has been less insisted on, though perhaps not less curious and deserving of attention.

* The land tortoise of the shores of the Mediterranean is another remarkable instance. I have mentioned how I found its urine to consist chiefly of lithic acid; yet its food is chiefly, indeed I believe, exclusively vegetable matter.

III.

ON THE ACRID FLUID SECRETED BY THE COMMON TOAD.

In every country in which the toad is found, it is considered poisonous by the common people; and the opinion may be traced back to a very remote antiquity. Of late years the notion has been rejected by the professed naturalists, and placed in the number of vulgar prejudices. Thus M. Cuvier, in his general remarks on the species, says "Ce sont des animaux d'une forme hideuse, dégoûtant, que l'on accuse mal-à-propos d'être venimeux par leur salive, leur morsure, leur urine, et même par l'humeur qu'ils transpirent."*

From observations which I made to endeavour to determine this point in the Ionian Islands, in 1825, I was obliged to come to the conclusion, that the popular opinion was not without foundation, and, of the two, the most correct. The results of my experiments on the subject, were communicated to the Royal Society, in the year in which

* "Le Règne Animal," tom. ii. p. 94. Paris, 1817. The remark is repeated in the last edition, that of 1827.

they were made, and they were published in the Philosophical Transactions for 1826; they were limited to the common toad, (Rana Bufo, L.) and were nearly as follows:

In carefully examining the toad, I found it possessed of a peculiar fluid, seated chiefly in the integuments, in follicles, in the cutis vera, beneath the cuticle and the coloured rete mucosum. These follicles were largest and most numerous near the shoulders and about the neck of the animal; but they were far from being confined to these parts, they were very generally distributed, and even on the extremities. Pressure being applied to the skin, a yellowish thick fluid, the fluid in question, exuded, occasionally it even spurted to a considerable distance. No difficulty was experienced in collecting it, from large and old animals, in sufficient quantity for examination. The following are some of the properties of the specimen which was subjected to experiment.

The greater part of it was soluble in alcohol and water. The aqueous solution was slightly viscid, and did not pass readily through a common filter. It was not precipitated by acetate of lead; and its transparency was very slightly impaired by a solution of corrosive sublimate. The substance obtained by evaporation, both from the aqueous and alcoholic solution, was light yellow and transparent; had a faint and peculiar smell different from that of the toad; and it was slightly bitter and very

acrid, acting on the tongue, like the extract of aconite prepared in vacuo; and even occasioning a smarting sensation when applied to the skin of the hand; its effect lasted two or three hours. When heated, it readily melted; burnt with a bright flame, and did not emit an ammoniacal odour. It did not appear to be either acid or alkaline, judging from its not changing the colour of turmeric, or of litmus paper. Caustic ammonia (*aqua ammoniæ*) dissolved it, without depriving it of its acrid quality. Nitric acid also dissolved it; forming a purple coloured solution, which neutralized by an alkali, was less acrid, as if partially decomposed. The small portion of the fluid, not soluble either in water or alcohol, and to which it owed its consistence, was probably a variety of albumen; and its appearance, when burning, would seem to warrant this idea.

Though this fluid of the toad was more acrid than the poison of the most venomous snakes, it did not appear to have any injurious, and much less fatal, effects, when absorbed and carried into the circulation of an animal on which it was tried: thus, a chicken, punctured with a lancet which had been dipped in it, received no apparent injury. And, in confirmation of this statement, I may remark, that although, as already observed, it abounds chiefly in the integuments, it is not confined to them; I found it in notable quantity in the bile, in minute

quantity in the viscid fluid lubricating the tongue, and also in the urine, and even in the blood.

In my paper, presented to the Royal Society, I called this fluid a poison; considering the properties described, perhaps it may with more propriety be called an acrid than a poisonous fluid; and rather analogous to that secreted by certain insects, as the bee, and scorpion, than to the more virulent and less acrid fluid of the poisonous snakes.

Reflecting on the use which this fluid may be of to the toad, it occurred to me, that it may answer two purposes, and these of importance to this abhorred but innocent reptile.

As the external surface of the skin is smeared with this "sweltered venom," as it has been called by our great dramatic poet, it must serve to defend it from the attacks of carnivorous animals: "a toad to eat," is a proverbial expression well known; and the facts adduced show its propriety and force. I may here add, that nature has given this sluggish and helpless animal an additional security against attack, in providing it with integuments of great thickness and strength, and hardness; which last-mentioned quality is imparted by a layer between the fibrous cutis, (the cutis vera) and rete mucosum, almost analogous to bone, abounding in phosphate and carbonate of lime, and carbonate of magnesia, semi-tranparent, and yet so firm, that it is not easily cut.*

* The tegumentary system of reptiles is curiously varied in struc-

As the fluid contains a substance which is very inflammable, and as it may be considered excrementitious, though the blood is very slightly impregnated with it, it may serve to separate a portion of carbon from the blood, and thus in its function be auxiliary

ture and composition; in every instance admirably adapted to the peculiar habits of the species. How great is the contrast between the soft mucous covering of aquatic active frogs, and the firm, hard, envenomed covering of the sluggish land-toads; or of the former, and the tortoise; or of the different species of lizards, compared one with another, in relation to their skin, in connexion with their habits, and defensive wants against their enemies. A careful chemical examination of the integuments of reptiles, I have no doubt, would amply repay and bring to light many curious and interesting facts. My own labours in this field have hitherto been very limited and partial. Besides the skin of the toad, I have only examined that of two other reptiles, viz. the python and Indian alligator. The first dry, I found to consist of

96·7 animal matter.
3·3 earthy matter.
―――
100·0

The earthy matter consisted of phosphate of lime and silica, the latter in large proportion, coloured reddish by peroxide of iron.

The second dry (a single plate from the back which weighed 88·2 grs. was the subject of experiment, and it was further dried at a temperature of about 212, previous to examination,) was found to contain

5·7 water (hygrometric)
33·6 earthy matter
60·7 animal matter.

Subjected to a strong heat in an open crucible, the animal part consumed with flame, and afforded a residue retaining the form of the plate, differing from the original only in being somewhat shrunk. This bony plate consisted principally of phosphate of lime, besides which it contained a little carbonate of lime and silica. On the inner surface of the plate, there were minute concretions of quartz, the largest about the size of a small pin's head, semi-transparent, but not distinctly crystalline. Their hardness, and not being acted on

to the function of the lungs. In support of this idea, I may remark, that I have found the pulmonary arteries of the toad, each divided into two branches; one of which went to its respective lung, and the other, very little smaller, to the cutis, between the head and shoulder on each side, and was extensively ramified where the largest follicles were situated, and where there was a plexus of veins of great size, as if intended for a reservoir of blood.

This last-mentioned peculiarity of structure, and the situation of it, corresponding to the gills of the tad-pole, would seem to indicate that the subcutaneous distribution of the second branch of the pulmonary artery, may aid the lungs further in their office by bringing the blood to the surface to be acted on by atmospheric air.

I have endeavoured to ascertain if there is any direct communication by spiracula through the integuments. The results obtained have been negative. Air has been introduced by means of a forcing syringe, under the loose skin, through a small incision, also into the cavity of the abdomen, and into the lungs by the superior glottis; and it has been much compressed, in these parts under water, yet it has been completely confined—not even the smallest

by an acid, were properties sufficiently characteristic of their nature.

Whether the presence of silica in these instances is to be considered as accidental, or the effect of disease, or constant and normal, analogous to what occurs in the protecting epidermis of some plants, remains to be ascertained.

bubble was visible—forced either through the skin or through the lungs.

When dried, the skin of the toad, I find, exhibits two kinds of pores; one kind, few in number, confined to the tuberosities over the shoulders, sufficiently large to receive a hog's bristle; the other kind, very numerous, scattered over the whole surface, and very minute. Both of them were best seen by holding the skin between the eye and a strong light; the smallest appeared as luminous points of a yellow hue, the largest as indistinct circles. Both were covered externally with transparent cuticle, and internally by a delicate surface of cellular tissue; some of the largest were also covered with rete mucosum; the smallest appeared to be destitute of this membrane: both, I believe, are destitute of the superficial layer or crust already mentioned, consisting chiefly of phosphate of lime.

Whether these apparent pores are the medium of communication between the blood in the subcutaneous capillary vessels and the atmosphere, or whether they are merely parts of the cutaneous apparatus of secretion and exudation, it is difficult to determine; it is not improbable that they perform both functions: the experiments of Spallanzani, and the very ingenious ones of Dr. Edwards, on what has been denominated cutaneous respiration, may be considered as demonstrative, that they perform at least the first-named function.

IV.

ON THE POISON OF THREE OF THE POISONOUS SNAKES OF CEYLON.

Of twenty different species of snakes which I had an opportunity of examining in Ceylon, four only belonged to the poisonous kind; namely, the Hooded snake, the Tic-polonga, the carawilla (as they are called by the natives), and the Trigonocephalus nigro-marginatus, (the Bodroo Pam of Russel *). All these, with the exception of the last, of which, from its extreme rareness, I could not procure a living specimen, I tried on animals, with a view to endeavour to ascertain what are the effects of their poison. Owing to an unavoidable interruption, to which medical officers are liable on service, the inquiry was not carried nearly so far as I could have wished: I was obliged to leave it in an unfinished state. However, as the results which I had obtained appeared to me to be of some value (as the results of all experiments carefully made and honestly related necessarily must be) I thought it right to introduce them into my work on the Interior of Ceylon, which was

* Russel on Indian Snakes, vol. i. p. 60.

published in 1821. From that account, the follow- is an extract. I am induced to give it, partly for the reason already assigned, and partly in consideration that the work referred to is scarce, and out of print; and, moreover, as it appears desirable to call attention to a subject of acknowledged interest and importance, obscure and mysterious in itself, and peculiarly requiring various and multiplied elucidation by experiments.

1.—*On the Poison of the Hooded Snake (Naia tripudians, Marrem.)*

Experiment 1.—The snake used in this experiment was about five feet long and about six inches in circumference in its broadest part. It appeared to be active and in good health. On the 30th of November, 1816, at Colombo, a full-grown hen was brought near it. After much threatening, the snake darted on the hen, and fixed its fangs in the skin, covering the lower part of the pectoral muscle. It kept its hold about two or three seconds, when I succeeded in shaking it off.

At the moment, the hen appeared to be but little affected; she seemed rather uneasy and restless, and was every now and then pecking the part bitten. Some corn being thrown on the ground, she ate only a very little. In two hours she was worse; but even then the action of the poison was not apparent from any remarkable symptom, merely from a certain degree of debility and languor, indicated by her

being easily caught, and by crouching when not disturbed. There was no swelling or appearance of inflammation round the punctured wound. Her temperature, ascertained before the experiment, and now by thermometer *in cloacâ*, was found to have fallen from 109° to 108°. In four hours she was much worse; her breathing had become quick and laborious; venous blood seemed to predominate in the circulation; the comb was bluish and turgid. There was very great prostration of strength; she was unable to stand. The sensorial functions were not apparently deranged; the pupil was rather dilated than contracted; there were no convulsions; no rigors. Several liquid dejections occurred, and some watery fluid was thrown up. Her temperature was now reduced to 106°. I was obliged to leave the house; a servant watched her; during my absence she expired, eight hours after she was bitten.

I examined the fowl the next morning, ten hours after death. Externally there was no appearance of swelling or of inflammation, or of any kind of change, not even immediately on the spot wounded. Beneath the skin, where the fangs had penetrated, there was much cellular membrane and a layer of fat, sufficient to have prevented the teeth reaching the muscle. The cellular membrane under the wound exhibited slight indistinct traces of inflammation. On the mucous membrane of the intestines there were a few red spots. With this exception, the abdominal viscera in general had no unusual

appearance. The brain exhibited no marks of disease. Both ventricles of the heart were empty, contracted and hard; the auricles contained coagulated blood. The lungs were unusually red, and the air-cells were full of serum, which, on pressure, flowed out copiously.

Experiment 2.—Three days after, a powerful cock was exposed to the same snake. The snake fastened on his comb, and kept its hold with its fangs for one or two seconds. A little blood flowed from the wound.

During the first four hours, the bird did not appear in the least unwell, walking about and eating as usual. After the fourth hour he began to droop and lose his strength: in eight hours he appeared very weak, could scarcely walk; his feathers were ruffled; his eyes nearly closed; his breathing laborious. In this state he continued about twenty-four hours without eating: he voided much green excrement. His temperature was reduced from 109·75, which it was before he was bitten, to 106°. About the twenty-eighth hour, he appeared a little better, but still refused to eat: this was in the evening; the next morning (he was in my bed-room in a large basket) I was awakened by his loud and vigorous crowing. He now appeared recovered; he had regained his appetite, and his temperature had risen to 108°.

Experiment 3.—The day following, the same snake was tried on two puppies, both of whom it bit. In neither instance was the health deranged; nor had its bite, sixteen days after, any effect on a small full-grown dog.

Experiment 4.—The snake used in this experiment, was of the same size nearly as the last;—it had been recently taken, and was active and vigorous. On the 13th of February, 1817, a young cock, about half-grown, was presented to it, and bitten on the breast;—the wound was so very slight, that it was only just perceptible. During the first hour, the bird walked about and ate, as if nothing ailed him. Then he gradually sickened; his feathers became ruffled; he ate very little, and remained stationary in one place. For a day and half, he continued growing slowly worse; on the morning of the second day, he was found lying down, breathing very quick, and apparently insensible. He expired about the forty-third hour from the time he was bitten.

On dissection, no diseased appearances could be detected, excepting, perhaps, in the lungs, which were a little redder than natural.

Experiment 5.—A few minutes after the last fowl was bitten, another young cock, of the same age nearly and size as the preceding, was exposed to the snake. The snake fastened on his thigh, and inflicted rather a severe wound, from which some blood

flowed. The cock became instantly lame, and in less than a minute was unable to stand. In about five minutes, his respiration became hurried and rather laborious; some alvine dejections took place. In about ten minutes he appeared to be in a comatose state; and for about five minutes he continued in this state,—his respiration gradually becoming more feeble and laboured. In seventeen minutes, when his breathing was hardly perceptible, he was seized with a convulsive fit, which, in the course of the next minute, returned four or five times, each less violent than the former,—and the last proved fatal.

The heart was immediately exposed: the auricles were found still acting. The lungs were redder than usual, and turgid with blood and serum. The brain, carefully examined, displayed no morbid appearance. The thigh bitten was slightly swollen; the skin surrounding the wound and the subjacent muscle were livid.

Experiment 6.—On the 29th of October, 1816, at Colombo, I assisted my friend, Mr. Finlayson, in examining a small hooded snake about two feet long, very soon after it was killed. We opened the poison-bag and tried the effects of its poison on a wild dove.

First, a little of it was applied to the conjunctiva of the eye. Immediately the eye-lid fell, and the eye closed as if from paralysis. The eye forced open,—the pupil appeared much dilated.

Next, the tongue was punctured with a lancet dipped in the poison. In a few seconds, the bird had lost the use of its legs; it fell, and in two or three minutes expired, without apparent suffering or any remarkable symptom.

The body was immediately examined. No visible lesion could be discovered in any of the organs.

According to the best of my recollection, it was on this occasion, that both my friend and myself tasted the poison of the Naia, applying a very minute quantity which had been taken from the poison-bag, the same as was used in the preceding experiments, to the tip of the tongue. It tasted slightly acrid: the impression was of short continuance; and besides this sensation no other effects were produced.*

2.—*On the Poison of the Carawilla (Trigonocephalus hypnale, Schlegel).*

Experiment 1.—The snake used in this experiment and the following, was about a foot long, just

* This last experiment was not described in my work. In 1821, my friend was alive and actively engaged in scientific pursuits in India. Unfortunately for science, he was prematurely cut off, shortly after, on his homeward voyage, by pulmonary disease, contracted in Ava, where he accompanied, as naturalist and medical officer, the political mission under the direction of Mr. Craufurd, the able historian of the Eastern Archipelago. Mr. Finlayson's Journal of the Expedition, published after his death, bears ample and equal proof of his zeal, talent for observation and reflection, and rich acquirements.

taken, and very active. At Colombo, in December, 1816, it bit a puppy, about two months old, in two places,—the side of the face, and the foot of one of the hind legs. Immediately, judging from his howling, the animal seemed to suffer much pain; he ran away, when liberated, on three legs, making no use of the wounded leg. A drop of blood came from each wound. In less than two minutes, the parts wounded began to swell and to discharge a thin reddish-brown ichor. In an hour, the swellings were very considerable, and the puppy was severely unwell, — crying piteously, lying down, and, when roused, hardly able to stand. In about twenty-four hours, the swelling had extended to the parts adjoining the wounds, having subsided where it first appeared. The wounds still discharged an ichorous fluid, and appeared slightly livid. The health of the dog was improved; he was able to eat and move about on three legs. In about forty-eight hours, the swelling had nearly disappeared, and the animal seemed to have recovered his health. The wounds had sphacelated extensively and deeply, and a purulent discharge had commenced. In twenty-four hours more, a slough had separated from the wounds, which were now large deep ulcers, of a healthy appearance, promising to heal readily by the common process of nature.

Experiment 2.—On the same day, another puppy was bitten by the same snake in the fore-foot. The

parts immediately swelled, and discharged an ichorous fluid. From the foot the swelling extended to the shoulder; and from the shoulder to the integuments of the chest; subsiding where it first appeared, as it spread beyond. The health of the dog was much deranged; for two days, it could stand with difficulty, and ate very little. It had frequent small bloody evacuations, as if the bowels were inflamed. At the end of three days there was a considerable improvement; a great part of the skin of the leg bitten was now livid, and on the fourth day sloughed off, exposing an extensive healthy ulcerating surface. On the fifth day, the animal was still sickly; the ulcer healed slowly, and the dog gradually recovered its health.

Experiment 3.—On the day following that on which the two preceding experiments had been made, a half-grown fowl was bit by the snake just above the left eye. The eye immediately closed; a watery fluid, like tears, flowed from it, and there was a similar discharge from the nostrils. When the eye-lid was forced open, the pupil contracted readily on the admission of stronger light. The opposite side of the face soon became considerably swollen. The fowl drooped, but never lost the use of its legs. It refused to eat, and was much purged; what was voided had a chalky appearance. On the third day, it seemed to recover a little, and ate a small quantity of grain. On the fourth day, it was found dead.

Under the skin of the part bitten, there was some coagulated blood, and the cellular membrane was discoloured. On the external surface of the heart, within the pericardium, there was a covering of coagulated lymph, of a reticulated appearance, strongly indicating recent inflammation of the organ. The lungs were rather redder than natural. The gall-bladder was distended with green bile. The intestines were not inflamed and the other viscera had a healthy appearance.

Experiment 4.—On the day following, another fowl, half-grown, was bitten by the snake in the comb, which bled pretty freely. The comb and the skin of the head swelled slightly. For twelve hours, the fowl was sickly, and ate very little. The next day the swelling had disappeared, and the fowl was well.

Experiment 5.—After a month's confinement, during which time the snake ate nothing, it appeared to be as active and as fierce as when first taken. It now bit a fowl, half-grown, on the side of the face, about a quarter of an inch below the eye. The surrounding skin immediately began to swell, and the eye to discharge a copious watery fluid. In a few hours the eye was greatly swollen and distended with effused coagulated blood. During the first twelve hours, the fowl seemed sickly, drooped, and ate nothing. It had numerous white alvine evacuations. In about twenty-four hours, its appetite returned, and it

seemed pretty well, notwithstanding the diseased state of the eye, the sight of which, it had lost from ulceration.

Experiment 6.—After another fortnight, spent fasting, and without apparent diminution of activity, the snake bit a large frog on the head. The skin became slightly swollen, and discharged a bloody sanies. The swelling extended from the head to the trunk. The animal died in about five hours.

3.—*On the Poison of the Tic-polonga (Vipera elegans, Daudin).*

Experiment 1.—This and the following experiments were made with a Tic-polonga, about four feet and half long and very thick. It had been just taken, and was in full vigour.

In February, 1816, at Colombo, the same fowl was exposed to this snake, that had lost an eye from the bite of the Carawilla. It seemed desirous to avoid the fowl, retiring and hissing with extraordinary shrillness and loudness; after being irritated very much, it darted at the fowl and struck it, but did not appear to have wounded it, though it really had, in the slightest manner possible, near the insertion of the great pectoral muscle. In about a minute, the fowl was seized with violent convulsions, which, in two or three seconds, terminated in death.

The chest was immediately opened. The two

auricles, and the right ventricle, and the great veins, and arteries, were distended with coagulated blood. When first exposed, the heart was motionless; after the removal of the pericardium, the auricles exhibited, for a few seconds, a slight tremulous action. In the brain, lungs, and other viscera, no diseased appearances could be detected. The vessels generally were distended with coagulated blood. The muscles were very flaccid, and could not be stimulated to the feeblest contraction, immediately after death; even the pectoral muscles, when divided, showed no signs of irritability, nor did the intestines. Where the wound had been inflicted, the skin and the muscle underneath were a little darker than natural, as if from a minute portion of extravasated blood. The muscular fibre was extremely soft and weak; slight pressure on the muscles occasioned the exudation of a little watery fluid, and some minute globules of air. There was not the slightest swelling of the part, or appearance of local inflammation.

Experiment 2.—About half an hour after the preceding experiment, a full-grown fowl was exposed to the snake. Even more provocation was required in this instance than in the first, to excite the snake to act;—at length he bit the fowl in the wing; the fangs penetrated the loose skin and drew a little blood. At the moment, the fowl did not appear to suffer any pain, or to be in any way affected. In about a quarter of a minute, by a second-watch, its

breathing had become accelerated; the eyes nearly closed and the pupils a little contracted. In a minute, the fowl was seized with convulsions of a very severe kind;—the head was bent down and fixed on the breast; the legs were drawn up; in brief, every muscle appeared to be in violent action and spasmodically contracted. The convulsions lasted about half a minute, when the fowl expired.

The appearances on dissection were the same as in the preceding instance. There was no discoloration or apparent change in the parts surrounding the wound.

Experiment 3.—Six days after, the snake bit a young dog, nearly full-grown, in the hind leg. A good deal of blood flowed from the wound. The dog immediately ran away, howling, with the foot drawn up.

During the first *ten* minutes, he was very restless; his movements were rather convulsive, particularly the motions of the hind-leg that was not bitten. Sometimes he would lie down and appear a little composed, and as suddenly start up and re-commence a piteous howling.

In about *fifteen* minutes his breathing became hurried, the muscles became spasmodically affected, and violently so, particularly those concerned in respiration; copious evacuations took place, *sursum et deorsum*. His strength was now much diminished; he lay on the ground breathing rapidly and

crying shrilly, every now and then starting up, as if from pain, and almost instantly falling again, as if from inability to support himself. In about *twenty* minutes, he was apparently almost exhausted ; the breathing was short and spasmodic, as it were by jerks, and amazingly rapid—about ninety in a minute ; each expiration was accompanied with a shrill sound, and now and then there was a full inspiration, followed by a deep groan. Even now, the sensorial powers seemed to be little affected ; the poor animal was conscious of what was passing, and when patted on the head seemed to be soothed.

In about *twenty-six* minutes he became insensible and apparently comatose, not to be roused by any means of excitement applied. The pupils were rather contracted ; respiration was quick, but not so quick as before, and more full : now and then it was more hurried, now and then there was a deep inspiration and a moan, his extremities were nearly cold.

In about *fifty-one* minutes his respiration became spasmodic, the head pulled down at each inspiration. The respiration became slower and slower, and gradually more feeble, till at the *fifty-eighth* minute it ceased entirely. The pupil was dilated ; just before death, there was a slight convulsive motion of the whole body.

The body was immediately examined. A good deal of serum was found effused on the surface of the brain, in its ventricles and at its base : the sur-

face of the brain (the pia mater) was redder than usual; there were air-bubbles in the vessels of the pia mater, and in the larger veins. The liver was very red, and gorged with blood; the lungs were rather red, condensed, and contained little air. The ventricles of the heart were distended with blood, the right ventricle especially; the right auricle was less distended, the left auricle was empty. The blood in the heart and veins was liquid; it did not coagulate on exposure to the air. There were red spots on the inner coat of the stomach. The other viscera exhibited nothing unusal. The foot bitten was very slightly swollen, owing to the effusion of a little serum and blood. The muscle, under the wound was nearly black, there were air-bubbles in the adjoining cellular membrane; there was no offensive smell from it: the body was still warm.

Experiment 4.—Thirty-four days after the last experiment, during which time the snake would eat nothing, it bit a rat. The animal immediately lay down motionless; its respiration became quick and convulsive, and after two or three slight convulsions of the body in general, it expired.

The body was immediately opened. The heart had ceased to contract; the auricles did not contract when punctured. The surface of the heart was unusually red and vascular, as if inflamed; its cavities were distended with blood. The lungs were rather redder than natural. The muscular fibre in general

had entirely lost its irritability. There were no marks of disease amongst the abdominal viscera.

The first experiment with this snake was made on the day it was brought to me, the 3d of February; the last, which I have now to describe, on the 27th and 28th of June, an interval of 146 days, which it had passed fasting. It had refused different kinds of food offered, and yet it looked now as well as ever, and appeared to be equally vigorous; and from the result of the following experiments, its poison seemed to be not exhausted or weakened, but concentrated and more terribly active. As I was obliged to leave Colombo at this time, I do not know how much longer the snake lived without eating.

Experiment 5.—On the 27th June, the snake bit a full grown fowl in the face. The wound was inflicted in an instant; the fangs were instantly disengaged. In a few seconds the fowl was convulsed; every muscle seemed to be thrown into violent spasmodic action; the head was drawn down on the breast, the legs were extended, and the animal, lying on its belly, was moved about rapidly and irregularly by a quick succession of those involuntary spasmodic actions of its muscles, till it expired, which it did in rather less than a minute from the instant it was wounded.

The body was immediately opened. The muscles divided by the scalpel did not shew the slightest signs of irritability, nor did the intestines; the heart

contracted very irregularly, and so very feebly, that it had no effect in propelling its contents. The auricles were gorged with blood, particularly the right; the left ventricle was empty, and the right contained only a small quantity of blood; the arteries and veins were full of blood, which had coagulated firmly, even in the minute branches. There was no unusual appearance in the brain, or in any of the other viscera, excepting the distended state of their blood-vessels. A reddish sanies oozed from the wound; the skin round it was discoloured, without swelling; the muscle under the skin was blackish and tender, as if severely bruised; the adjoining cellular membrane was slightly emphysematous.

Experiment 6.—On the following day, the snake wounded another full-grown fowl, inflicting with its fang, in the side of the face, a puncture only just perceptible. During the first minute and a half, the fowl did not appear to suffer in the least, when it was seized with violent convulsions, which proved fatal in about fifteen seconds.

The appearances on dissection, which was commenced immediately, were very similar to the last. The action of the heart was perhaps a little less feeble; the vermicular motion of the intestines had not entirely ceased, and the blood was not quite so firmly coagulated. All the cavities of the heart were empty, with the exception of the right auricle, which was distended with blood.

Conclusions and general Remarks.

From the experiments on the poison of the hooded snake, it would appear that its bite is not necessarily fatal to fowls; that the effect of the bite varies a good deal according to circumstances which it is not easy to calculate; that the poison is capable of being soon exhausted; that the symptoms produced by the poison, though not uniformly the same, pretty generally correspond; and that, in conjunction with the appearances on dissection, they seem to indicate that the lungs are the *principal* seat of the diseased action.

From the experiments on the bite of the carawilla, it would appear that it is rarely fatal to small animals; that its poison is not easily exhausted; that the symptoms produced by it are pretty uniform; that they are different from those produced by the poison of the hooded snake, the diseased action being more local and much more inflammatory—commencing in the part bitten, spreading progressively, losing its force as it extends, and probably never proving fatal, except it reach a vital organ.

From the experiments with the tic-polonga, it appears that its bite greatly exceeds in fatality that of the hooded snake, or of the carawilla, and that the action of its poison is different and peculiar. Judging from the symptoms and appearances on dissection, its poison seems to exert its influence primarily and principally on the blood and muscular

system, tending to coagulate the former and convulse and paralyze the latter. All the experiments seem to point directly to this conclusion, not excepting even the third, the peculiarities of which may be referred to the reaction, which it may be conceived took place, the animal being pretty large and strong, and not overpowered by the immediate effect of the poison.

From the whole of the preceding experiments, it may be inferred, reasoning from analogy, in relation to man, that the bite of the tic-polonga is most dangerous; that of the hooded snake is dangerous in a less degree; and that of the carawilla, least of all, and probably never fatal. The result of the inquiries which I made amongst the natives, though not very satisfactory, tended to confirm these conclusions: I found them generally of opinion, that the bite of the tic-polonga is unavoidably fatal, but that of the hooded snake only occasionally so. Perhaps they exaggerated a little in maintaining the first part of this statement: the latter part of it I believe to be quite correct; for I have seen several men who had recovered from the bite of the hooded snake, and I have heard of two or three only to whom it had proved fatal.

I regret that I have nothing original to offer respecting the medical treatment of the bites of these snakes. It was my intention to have made a series of experiments on the subject; indeed, the experiments which I have detailed were merely pre-

liminary to that inquiry, being instituted to smooth the way, to determine, if possible, the mode of action of each poison, and furnish data for inferring what are the pure effects of the poison, what of the powers of nature opposing its effects, and what those of the medicine administered. In no subject is discrimination more required than in this mysterious one; and, in no one perhaps has less judicious discrimination been used. It has often been taken for granted that the poison of all snakes is similar,—not differing in its kind, but only in its intensity of action; and, agreeably to this assumption, that the medicine useful in one instance must be serviceable in all. And, too often, medicines have got into repute as antidotes from being given in slight cases, in which recovery would have taken place without medical treatment,—beneficial changes that were due merely to the preservative powers of the constitution. The reputation that many Indian medicines, and especially that snake-stones have acquired, afford striking proof of the preceding remarks; of three different kinds of these stones which I have examined, one consisted of partially burnt bone, another of chalk, and the third principally of vegetable matter; this last resembled a bezoar. All of them (excepting the first, possessed of a slight absorbent power) were quite inert, and incapable of having any effect, exclusive of that which they might produce as

superstitious medicines, on the imagination of the patient.*

The probability is, that the poison of each different kind of snake is peculiar ; and that, when fully investigated, the effects of each will be found to require a peculiar mode of treatment, the nature of which can only be ascertained by actual experiment. In the mean time, I may remark, that fortunately for man, in this great obscurity, the immediate treatment of all poisoned wounds, merely locally considered, is simple and very similar. The obvious indications are, to extract the poison as much as possible, and as speedily as possible; and to endeavour to prevent its entering the circulation. The first indication is best accomplished by cutting out the part bitten, and scarifying the surrounding integument. If the person bitten want courage to do this, he should suck the wound well, and afterwards apply caustic, if at hand. The second indication may be tolerably fulfilled (if the part bitten admit of the application of a ligature) by tying a handkerchief very tightly just above the wound. Respecting the employment of oil, arsenic and eau de luce, having no experience of their effects,

* It is the opinion of the natives that these stones are found in the brain of snakes. From Sir Alexander Johnson, to whom I was indebted for the specimens I examined, I learnt that those of the first kind are manufactured by the monks of Manilla, who carry on a lucrative trade in them with the merchants of India.

I can offer no decided opinion. Oil seems to have been useful, both applied externally, and taken internally, in many instances of the bite of the viper; and arsenic seems to have done good in some instances of the bite of the hooded-snake. Eau de luce does not appear to have deserved the high character that was first given to it, indeed I am not aware of any satisfactory proof of its ever having been beneficial.

V.

ON THE STRUCTURE OF THE HEART OF BATRACHIAN ANIMALS, ESPECIALLY OF THE GENUS RANA OF LINNÆUS.

From the urinary organs of reptiles, my attention, whilst I was in Ceylon, was directed to other parts of their structure, and especially to the apparatus of circulation and respiration.

It was at that time commonly asserted by the highest authorities in comparative anatomy, and generally believed, that the animals belonging to the genus Rana, and indeed all those included in the natural order of Batrachians of Cuvier, in the conformation of their heart, differed from the other reptiles, which possess a bi-auricular heart; and resembled fishes,—which have only a single auricle as well as ventricle.

The close analogy in general structure which exists between the batrachians and the other reptiles, led me to doubt the accuracy of this conclusion; and observations which I had an opportunity of making first in Ceylon, and afterwards in 1821 and

1825, in the Ionian Islands, satisfied me that my doubt was well founded.

Selecting old and large animals, especially the toad, which in the Ionian Islands often attains a great size, I experienced but little difficulty in demonstrating the bi-auricular structure of its heart. It was best effected by making an incision into the ventricle, and inflating the organ with the blow-pipe. By this method, with the aid of fine probes, manipulating under water, it was clearly proved that the heart of the animal has two auricles, which are divided by a transparent membranous septum, possessing fibres, which appear to be muscular; that they communicate with the ventricle by a common and very short passage, and that they have no communication with each other, excepting in the passage above the valves common to both.

The same fact as to structure, I found might also be demonstrated, by blowing air through either of the two pulmonary veins which return the blood from the lungs to the heart; or, by inflating the sinuses in which the venæ cavæ terminate. By inflation of either of the pulmonary veins, the pulmonic auricle was distended, and not the systemic. By inflation of the sinuses of the venæ cavæ, the contrary effect was produced, shewing, at the same time, that the margin of the septum was qualified to act the part of a valve, and prevent the blood of one auricle from passing into the other.

But even supposing the margin of the septum

not fitted to perform the function of a valve, I apprehend, from what I have observed in watching the heart's action, that the blood from one auricle would not pass into the other, the contraction of the two auricles being synchronous, the auricles first contracting, next the body of the ventricle, and lastly that part of the ventricle of a conical shape, which may be considered almost as a second ventricle.

This last mentioned part (to notice it more particularly, as I believe it deserves) appeared from my dissections, to be separated from the body of the ventricle by three valves, of a semilunar form, one large and two small, and to have attached within it, to the side of its cavity, a pale fleshy projection, a moveable septum, above which it gave origin to four great arteries, namely, two pulmonary arteries and two aortæ, the latter considerably larger than the former, each provided with its own valve. The action of this part is not a little curious: when I have watched it, it did not appear to contract simultaneously, as a whole, but one-half first, and then the other, as if intended, in conjunction with the various anastomoses of the arterial system, to preserve a constant, though small and feeble current of blood, to supply all the parts of the body according to their various demands.

The same general structure of heart, I have observed in the bull-frog distinctly, and in the common frog also, especially its double auricle. Whether it exists in all the other species of the genus, I have

not ascertained; most probably it does. And reasoning from analogy, it appeared highly probable, that all the other genera of the order Batrachia, have a similar conformation, both of this vital organ, and of the sanguineous system in general.

I then remarked, should this analogical inference prove correct, and its truth be established by observation, these animals in their mature state would no longer be an anomaly in the classification of reptiles on account of their hearts, and they would still continue as a link connecting the reptiles with fishes,—the perennibranchiate, through the whole of their existence; the other species during the first stage of it, by the peculiarities of their respiratory organs.

This conjecture was offered in 1825, in a paper which was presented to the Royal Society, and which was afterwards published in the Edinburgh Journal of Science, edited by Professor Jameson; and I am happy to have to state, that it has been amply verified by the able and minute researches of Mr. Owen, as well as by those of other inquirers.*

I have stated, that when I began my inquiries, and first published my observations, all the batrachians were considered by the ablest comparative anatomists from the time of Harvey, downwards, as analogous to fishes in the auricular structure of

* Mr. Owen's observations are to be found in his Paper on the Structure of the Heart in the Perennibranchiate Batrachia, communicated to the Zoological Society, April, 1834.

the heart. And, I believe I was fully justified in the conclusion, by reference to their writings.* Since then, however, I have learnt, that our distinguished countryman, Mr. Hunter, was acquainted with the fact, that some of the batrachians have a bi-auricular heart, and that he made it in part the basis of a subdivision of the order. This I was informed of by Mr. Owen, who had the goodness to shew me a highly interesting document, preserved through the medium of a copy, a classification by our great physiologist, of the animal kingdom according to its organisation, and which Mr. Owen has in part given in his valuable prefatory remarks on the physiological labours and discoveries of Hunter, prefixed to his edition of this portion of his works.

At what time Hunter ascertained the fact, it is impossible now to determine, in consequence of the unfortunate destruction of his MS. papers: it is most likely that it was late in life, as there is no preparation in his splendid collection, in the Museum of the College of Surgeons, illustrative of it;—the only two preparations of the heart of the common toad and frog, left by him, seem from the

* According to Harvey, "piscibus, serpentibus, lacertulis, testudinibus, ranis et hujusnodi aliis, tum auricula, tum cordis ventriculus unus." Exercitatio Anatomica de Motu Cordis, cap. xvii.

According to Cuvier, "Les Betraciens n'ont au cœur q'une seule oreillette et un seul ventricule." Le Régne Animal, nouvelle édition, Paris, 1829.

descriptive catalogue published in 1831, designed to illustrate the commonly received idea.*

The circumstance that Hunter was acquainted with the bi-auricular structure in question, may serve to account for the adoption of it by Sir Everard Home. In his "Systema Régni Animalis," in the 3rd volume of his Lectures on Comparative Anatomy, he includes the genus Rana in the same class with Lizards and Snakes, to which he assigns "Cor uniloculare, bi-auritum, sanguine frigido rubro;"—but without specification there, or in any of his writings, of the observations by which the fact was determined.

* The following is the account given of two preparations alluded to in the text:—"175. The heart of a large toad (Rana Bufo) injected. It is composed of a single auricle and ventricle; from the latter, the arterial trunk or aorta arises, and immediately after its origin divides into two branches, which ultimately unite into one descending aorta, at the inferior part of the spine. Hunterian."

"176. The heart of a frog (Rana temporaria) injected. As in fishes, the heart is composed of two cavities, a single auricle, and a ventricle. Hunterian."

Catalogue of the Contents of the Museum of the Royal College of Surgeons in London, Part V. 1831. p. 16.

VI.

AN ACCOUNT OF SOME EXPERIMENTS ON ANIMAL HEAT.*

In the present uncertain state of our knowledge of animal heat, theoretically considered, three circumstances are particularly deserving of attention, viz. the relative capacities of venous and arterial blood for heat, their comparative temperatures, and the temperatures of different parts of the animal body.

On the first of these subjects, we possess only the experiments of Dr. Crawford, which I believe have not yet been repeated, notwithstanding they form the basis of his hypothesis.

On the second, little inquiry has been made, and especially of late years, since the improvement of the thermometer.

And, on the third, the observations that have been collected are very few in number, and with the exception of those of Messrs. Hunter and Carlisle, are scarcely perhaps deserving of confidence.

* This Paper first appeared in the Philosophical Transactions for 1814; most of the prefatory remarks are still applicable.

Such were the inducements that led me to the consideration of each of these subjects apart, and to endeavour to acquire by experiment some more certain knowledge respecting them. The experiments that I have made will be described in the two following sections, and in the last will be offered the few remarks and conclusions which naturally arise, and are fairly deducible from the results.

1.—*On the Capacities of Venous and Arterial Blood for Heat.*

I must premise, that my object has been to endeavour to ascertain the relative capacities of venous and arterial blood for heat, rather than their exact specific caloric. The latter, from many circumstances, is difficult to be accomplished; whilst the former is comparatively easy, and, in a theoretical point of view is probably equally useful.

I have employed both the methods commonly used. I shall mention most of the experiments that I have made, without noticing the repetitions of them, and shall begin with those on the times of cooling of equal volumes of venous and arterial blood.

The blood used was from the jugular vein, and the carotid artery, of a lamb about four months old. It was received in bottles, and to remove the fibrin, which is a great impediment in experiments of this kind, it was immediately stirred with a wooden rod. In respect to colour, the difference between the

venous and arterial blood was not so great as in the sheep's, and this in a great variety of instances I have always observed, the venous being of a less dark hue. The specific gravity of the venous blood, without the fibrin, was found to be 1050, and that of the arterial 1047.

A glass bottle, equal in capacity to 2518 grains of water, and weighing 1332 grains, was filled respectively with water and venous and arterial blood of the temperature of the room 62°, about four hours after the blood had been drawn, during which time each bottle had been closely corked. A delicate thermometer, by means of a perforated cork, was placed in the middle of the liquid. The bottle was then plunged into water of the temperature 140 Farenheit, and when the mercury had risen to 120, the bottle was quickly wiped, and suspended in the middle of the room; and the progress of cooling was noticed every five minutes till the thermometer had fallen to 80°. The following were the general results obtained.

Water cooled from 120° to 80° in	91	minutes.
Arterial blood in - - -	89	—
Venous blood in - -	88	—

Considering, therefore, the capacity of water for heat to be denoted by 1000, neglecting the effect of the glass bottle, producing a difference only of about half a minute, and the same in each instance, and dividing the times of cooling by the specific gravity,

the relative capacities of venous and arterial blood, without fibrin, appear to be as 921 and 934.

In the following experiments, the same kind of blood, and the same quantity, was used as in the preceding. The mixtures were made in a very thin glass receiver, containing a delicate thermometer. The temperature of the room was 66°.

Hot water, temperature 121°, cold water 61°. Mixture of the two 90°, after two minutes 89°, after three 88°, after eight 87°.

Venous blood 121°, water 62·5, mixture 89°, after three minutes 88·5, and after seven 87°.

Now, allowing about one degree of the cooling effect to have been produced by the receiver, indicated by the admixture of the hot and cold water, calculating the quantity of blood used from the knowledge of its volume and specific gravity, employing the formula given by Professor Robinson, which consists in multiplying respectively the weight of the water and the blood by the change of temperature, and dividing the first product by the second, the quotient or specific caloric for venous blood appears to be as 812, and for arterial, as 814, results very similar to those I have obtained with the blood of the sheep.

In the remaining experiments, blood with the fibrin present was employed, and with this exception they were perfectly similar to those already described.

The blood, used to ascertain its time of cooling,

was obtained from a sheep; and one day the vein was opened, and on the next the artery. The capacity of the bottle employed exceeded that of the first by one ounce measure of water; but it was equally thin. The air of the room was of temperature 69°.

Water cooled from 120° to 80° in 118 minutes.				
Venous blood in	-	-	112	-
Arterial blood in	-	-	113	-

And hence, as the latter was of specific gravity 1049, its capacity for heat seems to be as 913; and as the former was of specific gravity 1051, its capacity appears to be 903.

In the following experiment, equal volumes of fluid blood and of water were used, which was easily accomplished by means of a thin bottle with a large mouth, to which a cork was adapted, with a perforation more than sufficiently large to admit the bulb of a very delicate thermometer, and of course to allow, when the bottle was filled to the brim, the excess to flow out on the introduction of the cork, which was always similarly placed. To retard the process of cooling, the bottle was closely surrounded by a thick layer of what is commonly called cotton wool. Its capacity was equal to five ounce measures of water, or 2400 grains. It was first filled with cold water, which, when its temperature had been ascertained, was thrown into the receiver before used; it was next filled with hot water, of temperature about 110°, so that the heat of the glass might be nearly the same as that of the blood; and, lastly,

when the vein or artery had been opened, the bottle was immediately emptied and filled with blood—the temperature of which was ascertained by the thermometer in less than a quarter of a minute. The mixture was now instantly made, and by the same thermometer the highest temperature of the mixture was discovered.

The four following trials were made on the blood of two lambs, both about five months old. The temperature of the air was 60°.

Cold water 57·5. Venous blood 100°. Mixture 80°; after one minute 78·5.

Water 58°. Arterial blood 103°. Mixture 80°; after one minute 79°.

Water 58°. Venous blood 101·5. Mixture 79°; after one minute 78·25.

Water 58°. Arterial blood 106·5. Mixture 81°; after one minute 80°.

The rate of cooling was not noticed after the first minute had elapsed, as the blood then generally began to coagulate. The specific gravity was only ascertained in the two last trials; that of the venous blood was found to be 1050, and that of the arterial 1049; and hence, allowing, as before, one degree of the cooling effect to be produced by the receiver, the capacity of venous blood for heat appears to be 852, and that of arterial blood 839.

It is evident that these trials admit of less accuracy than the preceding; and much more confidence, it appears to me, is due to the third series of experi-

ments, so that, if required, I should be inclined to give the numbers thence deduced, as the greatest approximation to the truth.

2.—*On the Comparative Temperature of Venous and Arterial Blood, and of different Parts of the Animal Body.*

To endeavour to ascertain the comparative temperature of venous and arterial blood, I have made a considerable number of experiments; some of which, on lambs, sheep, and oxen, it will be sufficient for me in this place to describe. In each instance a long incision was made through the integuments, the jugular vein was laid bare, and the exact seat of the carotid artery found. The vein was then opened, and a small delicate thermometer introduced, and thrust about an inch up the vessel beyond the wounded part, and as the bulb of the instrument was small, the flow of blood was not stopped. When the mercury was stationary, its height was marked. The carotid artery next was divided, and the thermometer was immersed in the current of blood, and left there till it ceased to rise.

The following are the results of five experiments made on lambs, all of which were about three months old. The thermometer in the shade stood at 65°.

1.	Venous blood	102·5	Arterial blood	104°
2.	- -	104°	- -	105°
3.	- -	104°	- -	105°
4.	- -	103·5	- -	105°
5.	- -	104°	- -	105°

The following results were obtained from three experiments on sheep, the exact age of which I could not ascertain. The thermometer in the shade was 60°.

1.	Venous blood	103·5	Arterial blood	104°·5
2.	- -	102·5	- -	104°
3.	- -	103°	- -	104°

The experiments on oxen were only two in number. The temperature of the air was 64°.

1.	Venous blood	100°	Arterial blood	101·5
2.	- -	100°	- -	101°

In both instances the oxen were knocked down before the vessels were opened; and as respiration had ceased in consequence of the injury of the brain and spinal cord, no difference of colour, of course, was perceptible between the blood from the jugular vein, and that from the carotid artery.

These results, so different from what might have been expected, from the observations of Messrs. Coleman and Cooper, on the temperature of the two sides of the heart, led me to repeat their experiments. The experiments in which I place most confidence, were made on lambs, about four months old, and to these I shall confine myself at present. In each instance the animal was killed by the division of the great vessels of the neck; an opening was made immediately into the thorax, and a very delicate thermometer was introduced into the ventricles of the heart by means of a small incision. The operation occupied so short a space of time,

that in three instances the right auricle had not ceased contracting.

1.	Venous blood - -	104°
	Arterial - - -	105·5
	Rectum - - -	104°
	Right ventricle - -	105·5
	Left - - - -	106°
2.	Rectum - - -	105°
	Right ventricle - -	105°
	Left - - - -	106°
3.	Rectum - - -	105°
	Right ventricle - -	105·5
	Left - - - -	106·5
4.	Rectum - - -	105°
	Right ventricle - -	106°
	Left - - - -	107°

I cannot well explain the difference which exists between the results of the preceding experiments, and those of Messrs. Coleman and Cooper, which are directly opposite, excepting on the supposition of its being connected with the mode in which the animals they experimented upon were killed, viz.—by asphyxia. In death by asphyxia, there is generally an accumulation of blood in the right ventricle, and in many instances I have observed, when the right ventricle has been distended with blood, little difference of temperature between the two sides of the heart.*

* In instances of death by asphyxia,—theoretically considered, it might be expected, that the blood returning from the lungs to the

To describe all the experiments that I have made to ascertain the temperature of different parts of the animal body, appears superfluous, especially as the comparative results were very similar. It will be sufficient therefore here to notice the observations made on the human body, and on that of a lamb.

That the thermometer might be equally applied to all parts of the surface, its bulb, in form nearly cylindrical, was fixed to a small piece of cork, hollowed and lined with fine wool, and thus half its superficies was applied in each instance. The observations were made on the naked body at seven A.M. immediately after quitting bed. The temperature of the air of the room was 70°. The following were the results obtained.

At the central part of the sole of the foot	90°
Between the malleolus internus, and the insertion of the tendo achillis, where the artery is felt - - -	93°
Over the middle of the tibia - -	91·5
Over the middle of the calf - -	93°
Over the popliteal artery at the bend of the knee - - - - -	95°

left side of the heart, not having been acted on by air,—and there having been no production of heat, would necessarily resemble venous blood, in temperature as well as in colour. In this point of view the results of the experiments referred to above, may be considered confirmatory of the hypothesis in which the lungs are held to be the chief seat of the heating process.

Over the femoral artery in the middle of the thigh - - - - -	94°
Over the middle of the rectus muscle	91°
Over the great vessels in the groin -	96·5
About a quarter of an inch below the umbilicus - - - - -	95°
Over the sixth rib, on the left side, where the heart is felt pulsating - -	94°
Over the same place in the right -	93°
Under the axilla, the whole surface of the bulb being applied - - -	98°

About an hour had now elapsed from the commencement of the experiment. The thermometer again applied to the sole of the foot rose no higher than 85°, five degrees less than at first. A disagreeable sensation of cold was experienced, and particularly in those parts not supplied with large vessels, and out of the course of the great arteries. The body remained unpleasantly chilly till breakfast had been taken, and then a slight degree of pyrexia was perceived; the heat of surface being increased, the pulse quickened, and the mouth slightly parched. After breakfast the thermometer was applied to both hypochondriac regions, and the left was found one degree higher than the right.

To ascertain the temperature of different parts of the surface beneath the integuments, the bulb of a thermometer was introduced through small incisions about half an inch between the skin and subjacent parts of a lamb just dead. The heat of the rectum

was first ascertained, as a means of marking the rate of cooling, and the different parts were then tried in the following order:

Venous blood in the jugular vein -	105·5
Arterial blood from the carotid artery	107°
Rectum - - - - - -	105·5
Over the metatarsal bone - -	97°
Over the tarsal bone - - -	90°
Over the knee joint - - -	102°
About the head of the thigh - -	103°
At the groin - - - -	104°

Nearly a quarter of an hour had been occupied in making these observations, and the temperature of the rectum was now found to be 105°. The three great cavities were next opened in the order enumerated.

Near the lower part of the liver -	106°
The substance of the liver - -	106·5
The substance of the lung - -	106·5
The left ventricle - - -	107°
The right ventricle - - -	106°
The central substance of the brain -	104°
Rectum - - - - -	104·5

Surprised at the temperature of the brain being lower than that of the rectum, I was led to repeat the experiment. It may be proper to notice a few of the results, as it is a curious circumstance which they confirm. The four experiments I shall mention, were made on lambs. As soon as the animal was dead, the cranium was perforated, and a deli-

cate thermometer introduced into the central part of the brain.

1. Brain	-	104°	Rectum	-	104·75
2. Brain	-	105·75	Rectum	-	105·5
3. Brain	-	105·5	Rectum	-	106·5

4. Posterior part of the brain 105·5. Anterior 103°. Rectum 106·5.

The temperature of the air at the time was 68°. Different parts of the brain were found to vary considerably in temperature; the anterior, as already noticed, being lower than the posterior, and the superficial than the deep-seated parts.

3.—*Remarks and Conclusions.*

That there is no material difference between venous and arterial blood in respect to specific caloric, excepting what arises from difference of specific gravity; that the temperature of arterial blood is higher than that of venous; and the temperature of the left side of the heart than that of the right; and lastly, that the temperature of parts diminishes as the distance of parts from the heart increases,—are the general results of the preceding experiments.

Admitting the accuracy of these experiments, and I think that they will be found correct when repeated, what are their consequences in a theoretical point of view?

They are evidently in direct opposition to Dr. Crawford's hypothesis, the essence of which is, that

the capacity of arterial blood for heat is greater than that of venous, that there is no difference of temperature between the two ventricles of the heart, and in fact that the heat of all parts is nearly the same.

They are more agreeable to, and even support the hypothesis of Dr. Black, that animal heat is produced in the lungs, and distributed over the whole system by means of the arterial blood.

Neither are they inconsistent with that hypothesis which considers the production of animal heat as dependent on the energy of the nervous system, and arising from all the vital actions constantly occurring.

Besides the results of the preceding experiments, many arguments may be advanced in opposition to Dr. Crawford's hypothesis.

As we never perceive a difference of capacity in bodies without a difference of form or composition; and as very slight differences of the former result only from great changes of the latter, it might be expected *a priori*, as no difference excepting that of colour has been detected between venous and arterial blood, that their specific caloric would be very similar. From analogy also it might have been expected that the capacity of arterial blood for heat would be much less than that of water, as water appears to exceed almost every other fluid, and as the capacity appears to diminish as the inflammability of compounds increases. But the strongest

arguments against this hypothesis are to be derived from the experiments of MM. Delaroche and Berard.*

Dr. Black's hypothesis appears to me far more satisfactory than Dr. Crawford's, and capable of explaining a much greater number of phenomena.

The last hypothesis, which I mentioned, that which refers animal heat to vital action, has many facts in its support.

It may be said that the viscera of the thorax and abdomen are of the highest temperature, because these parts, are as it were, the elaboratories of life; and that the heat of the arterial blood, and of the parts best supplied with this fluid, is greatest, because they lie deepest, and abound most in the principle of life or vital action. This explanation was suggested to me by my brother, Sir H. Davy. There are some facts which I have observed agreeable to it, but not more so than to the hypothesis of Dr. Black.

I have found the stomach of the ox, the pyloric compartment, of a higher temperature than the left ventricle itself; thus, when the latter immediately after death was 103°, the former, full of food, was 104·5. I have also found the temperature of young

* On the specific caloric of gases; from which it would appear that the capacity of carbonic acid gas for heat, is actually higher than that of atmospheric air; or in other words, that the capacity of the air expired is greater than of the air inspired; and consequently, in this point of view, respiration ought to be, as it was considered by the ancients, a cooling process.—*An. de Chim.* lxxxv.

animals, in whom all the vital actions are most energetic, higher than that of animals arrived at maturity. I may mention here, in illustration of this statement, a few observations made on infants, as I am not acquainted with any yet published. In one instance I found the heat under the axilla of a child just born 98·5; after twelve hours 99°; and after three days, the same, during the whole of which time it appeared in perfect health.

On five other children of the same age, similar observations were made. In two instances of weak infants, the temperature, one hour after birth, was found not to exceed 96°, which is two degrees below the standard heat of man in a state of health; but their respiration was still languid, and the next day the heat of the axilla had risen in one to 98·5, and in the other to 99°.

To conclude; as in each hypothesis examined, difficulties are found to exist from facts or the results of experiments of an unbending nature, we must at present either suspend theory altogether and search for *experimenta crucis*, or adopt that hypothesis which is conformable to the greater number of facts. The first measure is certainly most philosophical; but to the latter we are naturally most inclined; and if I were questioned which view is preferable, I should not hesitate in selecting Dr. Black's, which to me appears both most simple and most satisfactory.

VII.

OBSERVATIONS ON THE TEMPERATURE OF THE BRAIN COMPARED WITH SOME OTHER PARTS OF THE BODY.

From the results of some observations already detailed (p. 153), it would appear that the temperature of the brain is not quite so high as that of the rectum, in the instance of sheep, and that the superficial portion of the brain is less warm than the deeply seated.

Professor Müller, in his Elements of Physiology, has called in question the accuracy of these observations, and has rejected the conclusion to which they led.

I have been induced, in consequence, to make recently some additional trials on sheep, which I shall now relate.

1.—December 4th, 1837, air 44°. The head of a two-years-old wether was cut off, immediately after death, by the division of the great vessels of the neck. A thermometer, plunged into the base of the brain through the foramen magnum, rose to - - - 104°

Forced in further, so as to be near the surface of the cerebrum, it fell to	-	103°
Now introduced into the rectum, it rose to		105°
2.—An ewe two years old; thermometer in recto before being killed -	-	106°
After decapitation, at base of brain	-	105·5
- - near the surface	-	104°
3.—January 22, 1838; temperature of air 45°. Immediately after decapitation, base of brain - -	-	105°
Rectum - - -	-	106°
4.—Before decapitation, rectum	-	103·5
After decapitation, brain -	-	103°
5.—Before - rectum -	-	104°
After - base of brain	-	105°
- - near surface of cerebrum	-	103°
6.—Before - rectum -	-	105°
After - base of brain	-	105°
- - near surface of cerebrum -	-	104°
7.—Before - rectum -	-	104°
After - base of brain	-	105°
- - near surface of cerebrum	-	104°
8.—Rectum, before decapitation	-	105°
Venous blood; the thermometer placed in jugular vein, the blood flowing freely		103°
In current of arterial blood from carotid artery - - -	-	105·5

In current of mixed blood, from the great vessels of the neck divided in the ordinary manner - - -	104°
Brain at base, after decapitation - -	105°
Near the surface of cerebrum -	104°
Right ventricle (some blood in it) -	106°
Left ventricle (empty) - -	108°
Liver - - - -	107°
Crystalline humour of the eye -	102°

The subjects of these observations, of the 22nd January, were virgin ewes between two and three years old, with the exception of No. 7, which was a wether.

These results are generally confirmatory of the preceding, although not without exception in relation to the base of the brain. In another place, further on, I shall have occasion to give some additional observations on this point.

That the temperature of the surface of the brain at least should be uniformly less than that of the rectum, appears to me what might be expected *à priori*, considering the comparatively small supply of blood to that part, and its exposure to cooling influences. In the instance of birds, the effect is very strikingly exemplified; thus, in the turkey, on the 22d February, when the thermometer in the open air was 34° and the room 46°, in cloaca it was 108°, in the gizzard 109°, under the pectoral muscle 108°, and in the brain 90°. The temperature in cloaca was ascertained during life; the other observations

were made as soon as possible after death; 1st, in the brain; 2d, under the pectoral muscle, and lastly, in the stomach.

The effect of cooling influences on the brain is shewn in a very decided manner in the instance of the brain of man after death. Excepting the hands and feet, the brain appears to lose its heat more rapidly than any other part of the body. Some precise observations on the temperature of the brain after death, compared with that of other parts, warrant this remark: they will be given in detail in the sequel.

VIII.

OBSERVATIONS ON THE TEMPERATURE OF MAN AND OTHER ANIMALS.*

In this place I shall give the results of some inquiries, which, chiefly between 1816 and 1820, I instituted on the temperature of man and other animals,—a subject, in a physiological, and in relation to man, in a pathological point of view, deserving, I believe, of more minute attention than, to the best of my knowledge, it had then received.

1st. I shall describe the observations I have collected, to ascertain the variation of temperature to which man is liable, in passing from the temperate into the torrid zone; in descending from a cool mountainous district into a hot low country; and in inhabiting a region where the diurnal vicissitudes of temperature are considerable.

2dly. I shall give an account of the attempts I have made to ascertain the temperature of different races of men.

* The observations of an earlier date than 1820, were first published in the Edinburgh Philosophical Journal for January, 1826.

3dly. I shall relate the results of my experiments on the temperature of different kinds of animals.

And, I shall conclude, with drawing such inferences as the premises may seem to warrant, and with making a few remarks on animal heat, as a speculative question.

1.—*Of the Variable Temperature of Man.*

In a voyage from England to Ceylon, in the year 1816, I had an opportunity of observing the effect of passing from one zone to another on the temperature of man.

It was in winter, in the month of February, that we set sail from England. I commenced my observations in March, when we began to experience the tropical heat; and on the 10th of the month, when our ship was in latitude N. 9° 42′, the weather fine, an agreeable breeze blowing, and when Fahrenheit's thermometer, exactly at noon, under an awning, where the passengers were assembled, was 78°. The gentlemen who were so obliging as to allow me to try their temperature, were all in good health, had breakfasted about three hours before, had taken little exercise, and though warm in respect to sensation, they were not disagreeably so, or sensibly perspiring. In each instance the temperature was ascertained, by placing a delicate thermometer under the tongue, near its root, every precaution being taken to insure accuracy. The following were found to be the temperatures of seven different gentlemen:—

No.	Age.	Temp.
1	24	99°
2	28	99·5
3	25	98·75
4	17	99°
5	25	99°
6	20	98°
7	28	98·75

On the 21st of March, in latitude N. 0° 12′, at noon, when the sun was apparently vertical, the sky clear, a fresh breeze blowing, and the temperature of the air 79·5. I repeated my observations on the same gentlemen, enjoying good health as before, and not unpleasantly warm.

No.	Temp.
1	100°
2	99·5
3	98·5
4	99°
5	99°
6	99·5
7	99°

On the 4th of April, in latitude S. 23° 44′, at between twelve o'clock and one in the afternoon, when the weather was very fine, a gentle breeze blowing, and the temperature of the air 80°, I repeated my observations on the preceding gentlemen, and on four more, and on a little girl and a boy. The circumstances were favourable, much the same as those already described, and the individuals not

unusually warm, though our sensation of heat was rather more than was agreeable.

No.	Age.	Temp.
1	-	99·5
2	-	99·5
3	-	99·75
4	-	100°
5	-	99·5
6	-	100°
7	-	99·5
8	25	101°
9	40	99·75
10	43	99°
11	40	99·5
12	13	100°
13	4	99·5

Lastly, on the 5th of May, in latitude S. 35° 22, after having been three weeks between this latitude and that of 30°, the weather damp and cool, I repeated my observations on a few of the same gentlemen as before, and at noon when the temperature of the air was 60°, and when we felt cool, almost cold.

No.	Temp.
1	98·5
3	98·25
5	98°
6	98·75
7	98·25
8	98°

I have had an opportunity of observing the effect

of the sudden change of temperature on the heat of man in descending from Kandy to Trincomalie.

The town of Kandy, the capital of the interior of Ceylon, is situated in latitude N. 7° 17′, and is elevated about 1,500 feet above the level of the sea.* Trincomalie is situated in latitude 8° 34′. Kandy is surrounded by hills and mountains, which are covered with wood, and frequently enveloped in clouds, and which abound in springs and torrents. Trincomalie is, at least, fifty miles distant from any mountains. The intervening country is low, and, though wooded, very dry, being subject to long-continued drought. One of the consequences of these peculiarities of situation is, that the difference of temperature, between the two places, is very considerable. The mean annual temperature of Kandy is about 73·5, whilst that of Trincomalie is about 10 degrees higher; and in the summer and autumn months the difference of temperature is from 12 to 15 degrees.

On the 15th of September, 1818, the day before I left Kandy for Trincomalie, at eight o'clock in the morning, when the air was 69°, I ascertained the temperature, both under the tongue, and in the axilla, of six persons who were to accompany me,—one a servant, the other five, part of a set of palankeen bearers, all natives of the western coast of the island, all in good health, cool, and fasting.

* This elevation I ascertained by means of the barometer. For the exact latitude of Kandy I was indebted to G. Lusignan, Esq.

No.	Age.	Temp. under Tongue.	Temp. in Axilla.
1	35	98°	96°
2	20	98°	97°
3	40	99°	97°
4	35	98°	97·5
5	20	98°	97·5
6	24	98°	97°

On the 3rd of October, the day after our arrival at Trincomalie, at nine o'clock in the morning, when the temperature of the air was 83°, I repeated my observations on the same men, who had not breakfasted, were in good health, and were not fatigued, having come the last fourteen miles the day before by water.

No.	Temp. under Tongue	Temp. in Axilla.
1	99°	97°
2	99°	97·25
3	99°	97°
4	99·75	99°
5	99·5	99°
6	99·5	98°

Again, on the 19th October, the day before we set out on our return from Kandy, at half-past eleven o'clock in the morning, when the temperature of the air was 82°, I renewed my observations on the men, who, since the 3rd of the month, had been leading

an idle life at Trincomalie; they had breakfasted about two hours before, and none of them seemed to feel disagreeably warm.

No.	Temp. under Tongue.	Temp. in Axilla.
1	102°	99°
2	101°	98·75
3	98·5	97·5
4	99°	98°
5	99°	98°
6	100°	99°

On the 28th of October, two days after our return to Kandy, at half-past eleven o'clock in the morning, I tried, for the last time, the temperature of these men, with the exception of two who were absent. They were in good health, though hardly recovered from the fatigue of a rapid harassing journey, in cool wet weather, through a country on the eve of breaking out into rebellion, the temperature of the air, at that time, had suddenly risen to 84° from 69°, which it was at seven A.M.

No.	Temp. under Tongue.	Temp. in Axilla.
1	98·5	99°
2	98°	97°
3	98°	97·75
4	98°	97·5

Kandy, from its peculiar situation, so near the equator, nearly in the middle of a large island,

elevated as it is above the level of the sea, and surrounded by mountains, is subject to considerable vicissitudes of temperature in the course of the day, and, consequently, is a place well adapted for making observations on the effects of these vicissitudes on animal heat. In fine weather the temperature of the air, at sun-rise, is always below 70°, and I have seen it as low as 55°; and in the afternoon, in such weather, it is always above 76°, and frequently as high as 83°.

On the 18th of January, 1818, a favourable day, I tried the temperature of an individual, at different hours, and obtained the following results.

Hour.	Temp. of Air.	Temp. under Tongue.	Sensation.
6 A.M.	60·5	98°	Cool.
9	66°	97·5	Cold.
1 P.M.	78°	98·5	Cool.
4	79°	98·5	Warm.
6	71°	99°	Warm.
11	69°	98°	Cool.

It may be proper to mention how the individual passed the day. He rose at six A.M.; read till nine A.M.; breakfasted temperately at ten; was engaged in some chemical experiments from half-past ten to two P.M.; from two P.M. to five was employed chiefly in reading; from five to six took gentle horse exercise; dined sparingly between seven and eight;

drank only one glass of wine; and, lastly, from nine to eleven was most of the time employed in writing.

It would be tedious to give other instances illustrative of the change of the temperature of man,—increasing with the temperature of the air, and falling as the atmosphere cools within certain bounds. The preceding instance, which has been confirmed by various experiments I have made, is the most minute and satisfactory that I can bring forward. The subject is inconvenient to make experiments on, and particularly for a person whose time is not his own, and, as a professional man, has seldom a day of leisure, the whole of which he can spend as he chooses. Nor is it easy in this inquiry to arrive at accurate results,—at any thing more than an approximation to the truth,—in consequence of the effects of a number of circumstances, and particularly of health, diet, and exercise, which cannot be duly appreciated until they have been more minutely investigated.

2.—*Of the Temperature of different Races of Men.*

At the Cape of Good Hope, at the Isle of France, and in Ceylon, I have had opportunities of trying the temperature of several different races of men.

At the Cape, in the winter of 1816, on the 24th of May, at noon, when the temperature of the air was about 60°, I prevailed, with some difficulty, on five Hottentots, to allow me to put a thermometer into their mouths, for they were afraid of the instru-

ment; and, when I saw them again, one blamed it for an illness with which he was seized soon after submitting to the experiment. I found their temperatures the following :—

No.	Temp. under Tongue.
1	98°
2	96·5
3	96.5
4	97·75
5	99·5

These Hottentots, I may remark, were in the service of our government, employed as artillery drivers. They were in good health, and resting at their barrack at the time. Their ages varied from twenty-five to forty, judging from their looks, for of the years they had lived, they themselves had taken no account. Those whose temperatures were lowest, I may mention, were most meagre and wretched in appearance; and, indeed, with the exception of No. 5, who was pretty robust, they were all of a very spare habit of body.

At the same time I ascertained the temperature under the tongue of three English artillerymen, in good health and cool, who had served ten years at the Cape, and who were giants in appearance when compared with the poor Hottentots. All three were nearly of the same age, between thirty and forty.

The temperature of one of them was 99·5, of the other two 99°.

At the Isle of France, at Port Louis, in June, one of the coolest months there, when the thermometer was at 74°, I tried the temperature of three Negroes, two natives of Madagascar, and one of Mozambique. Of these the temperature of the two first was 98°, and of the last 99°. They were house slaves, between eighteen and twenty years old, well clothed and fed, and in good health.

The temperature of an English gentleman, who had been in the island several years, and of another just arrived, was ascertained at the same time. The former was 98·25, the latter 98·5.

In the island of Ceylon I commenced my observations, at Colombo. Colombo, in latitude N. 6° 56′, is situated on the sea-shore, at the distance of about thirty miles from the boundary mountains of the Kandian country. Its temperature is remarkably equal; in the hottest day seldom exceeding 84°, and in the coolest night rarely falling to 70°. The greater part of the year, the range of the thermometer is from 77° to 73°, and the mean annual temperature is about 79°.

On the 14th of September, between six and seven o'clock in the morning, in a village about a mile from Colombo, when the air was about 79°, I tried the temperature of six Singalese, of different sexes and ages, all cool and in good health and fasting.

No.	Sex.	Age.	Temp. under Tongue.
1	F	50	101°
2	-	4	101·5
3	M	29	101°
4	-	8	101·5
5	-	40	100°
6	-	25	100°

These people lived in the midst of a cocoa-nut grove, and, like the Singalese in general, led an easy and indolent life, according to our notions of activity, and subsisted chiefly on rice, fruit, and vegetables.

The following morning, about the same time, and when the temperature of the air was the same, I tried the temperature of four Albinoes.

No.	Sex.	Age.	Temp. under Tongue.
1	F	5	101·5
2	-	12	101·5
3	-	23	101·75
4	M	27	101°

These Albinoes were the children of black parents; the two first were sisters, and they had brothers and sisters of the colour of their parents. They were all well made, active, and in good health.*

* The young Albino, twelve years of age, in England, and certainly in Norway, would not be considered peculiar; for her eyes

On the 12th of October, between six and seven o'clock in the morning, while the air was between 77° and 79°, I tried the temperature of a number of children, at the orphan school, in the neighbourhood of Colombo, some half caste, of Singalese mothers by English soldiers, others white of English parents.

HALF CASTE.

No.	Sex.	Age.	Temp. under Tongue.	Temp. in. Axilla.
1	F	12	100·5	98·5
2	-	14	101°	
3	-	17	100°	
4	M	14	102°	100°
5	-	10	101·5	99·5
6	-	14	100°	99°
7	-	10	100°	99°

WHITE CHILDREN.

1	F	9	101°	99·5
2	-	6	101°	98°
3	-	9	101°	98·5
4	-	12	102°	100°
5	M	8	102°	100°

were light blue, and not particularly weak, her hair of the colour that usually accompany such eyes, and her complexion fresh and rather rosy. She had considerable pretensions to beauty, and was not without admirers amongst her countrymen. It is easy to conceive that an accidental variety of the kind might propagate, and that the white race of mankind is sprung from such an accidental variety. The Hindus are of this opinion, and there is a tradition or story amongst them, in which this origin is assigned us.

These girls and boys at the excellent institutions to which they belonged, were well clothed and fed, as well as usefully educated. At the time, they were cool, and in good health, and had not breakfasted.

In the Kandian country, the climate of which in general very much resembles that of its capital, I have at different times ascertained the temperature of Kandians, Vaidas, Caffres, Malays, Sepoys, and Englishmen.

In Saffragam, a Kandian province, on the 17th of April, 1817, when the temperature of the air was 72° at seven o'clock in the morning, I tried the temperature of an old Kandian, almost a century old, and of a boy about 12 years old, both cool, but not cold :

Old man. Temp. under tongue 95°. In axilla 93°.

Boy. Temp. under tongue 98°. In axilla 96·5.

In Dombera, another Kandian province, on the 5th of September, at one o'clock in the afternoon, when the temperature of the air was 76°, I tried the temperature of three Kandians, stout men in the prime of life.

No.	Age.	Temp. under Tongue.	Temp. in Axilla.
1	24	99°	98°
2	30	99°	98°
3	33	98°	97·5

On the 7th of the same month, and in the same

mountainous district, I tried the temperature of three Kandian priests;

No.	Age.	Temp. under Tongue.
1	15	99°
2	16	99°
3	30	98°

At Kandy on the 7th of February, 1818, I tried the temperature of two young priests at five o'clock in the evening, when the air was 75°.

No.	Age.	Temp. under Tongue.
1	15	99°
2	16	98·5

In the same year, at Kandy, on the 29th of October, I found the temperature of a very old priest, (about a century old) in the axilla 98·5.*

The higher castes of Kandians, I may remark, to which the few subjects of my experiments belonged, are, for Indians, not only well formed, but stout and muscular men. Their food consists chiefly of rice and farinaceous fruits, which they use highly seasoned, and of milk, fowls and game. Their drink is principally water, the use of intoxicating liquors being contrary to their religion. Their ordinary dress is a handkerchief about the head, and a large cloth folded about the loins, and reaching below the

* Advanced old age in this person was well marked. He complained of all his senses having become impaired, excepting that of taste; he preferred animal food to vegetable, and the most savory meats, and ate often. He said he slept little, and dreamed much; and that his dreams were about the dead.

knee, with the addition, in cold weather, of another cloth, which is thrown over the shoulders, and wrapped about the body. Their dwellings are comfortable cottages. Their occupations chiefly agricultural pursuits. As they are stouter than the lowlanders, so are they more active, and as it appears to me, more acute and intelligent.

All the priests whose temperatures I tried, I should observe, were priests of Boodho, who dress and live in a manner peculiar to themselves. Their dress consists of yellow robes, which, thrown over the left shoulder, and girded about the loins, fall in graceful drapery to the feet, covering every part, with the exception of the neck, right arm and shoulder. They wear nothing on their head, which, as well as the eye-brows and the hairy part of the face, is carefully shaved and kept bare. They profess celibacy, lead an indolent quiet life, devoted chiefly to religious duties and literary pursuits, (such as they are) and subsist almost entirely on vegetable food.

At Kandy on the 12th of September, 1818, I had an opportunity which rarely occurs, of ascertaining the temperature of three Vaidas. The temperature of the air at the time was about 78°.

No.	Age.	Temp. under Tongue.	Temp. in Axilla.
1	60	98°	95°
2	30	98°	96°
3	35	98·5	96°

The ages of these men I was obliged to guess, for they themselves could not inform me. They belonged to a large party who had come to Kandy, with a tribute of dried deer's flesh and wild honey. They were quite naked with the exception of the *partes naturales*, which were concealed by a scrap of cloth. The hair of their head and beard was long and matted, and had never been cut or combed. Their eyes were lively, wild, and restless. They were well made and muscular, but of a spare habit, and in person they chiefly differed from the Kandians in the slightness of their limbs, the wildness of their looks, and their savage appearance. According to their own account of themselves, they came from the neighbourhood of the Lake of Bintenne, where they subsisted on game which they killed in the chase, some roots, and wild fruits, and a little grain of their own growing. They were profoundly ignorant, could not count above five, were hardly acquainted with the rudiments of any art, and though they feared demons as they did wild beasts, they had no knowledge whatever of a Supreme beneficent Being, and not the slightest notion of any state of existence after the present. Yet, these men considered themselves civilized in comparison with the wilder tribes of Vaidas, who never leave their native forests, and who attack with their sylvan weapons, the bow and arrow, every intruder into their haunts, and whom I have heard Kandians of a bordering province describe as living almost entirely

on raw animal food, as going quite naked, as having no superstition, and in fact as being in a state very little removed from that of brutes.*

On the 17th of December, 1818, when the air was 74°, I tried the temperature of five African negroes, servants in the Military Hospital at Kandy.

No.	Age.	Temp. under tongue.	Temp. in Axilla.
1	23	98·5	98°
2	35	98·5	98°
3	25	99°	98°
4	34	99·5	98°
5	28	99·5	98°

The ages of these men I conjectured from their looks. Most of them were from Goa. They were of African parents, had not degenerated, and like African negroes in general, they were stout and muscular. Nos. 4 and 5, I should remark, whose temperatures exceeded the rest, were in a state of gentle perspiration produced by slight exercise.

On the 18th of March, 1818, at noon, air 81°, at Kandy, I tried the temperature of four Malays :—

No.	Age.	Temp. under tongue.	Temp. in Axilla.
1	17	98·5	98·5
2	35	99·5	97·5
3	22	99°	98°
4	18	98·5	97·5

* The influence of habitual exercise in strengthening any particular set of muscles, is remarkably illustrated in the Vaida. I saw

These men were free Malays, in good circumstances. Three were natives of Colombo, and one of Cochin. They were active, stout, well made, and very muscular men, all of Javanese parents. They were dressed not unlike Kandians, but with less cloth about their loins, and a cloth most commonly over their shoulders.

On the 18th of May, in the afternoon, when the temperature of the air at Kandy was 80°, I tried the temperature of six Sepoys, belonging to a battalion of Madras native infantry:

No.	Age.	Temp. under tongue.	Temp. in Axilla.
1	25	98·5	98°
2	19	99°	96°
3	26	98·5	97°
4	22	98°	95°
5	38	100°	97°
6	20	98°	97°

Most of these Sepoys were natives of Madras, or of the adjoining country. They were tall, thin, and rather feeble men. They had been in Ceylon about three months.

On the 20th of the same month at between eight and nine o'clock in the morning, when the tem-

one, a young man, of a diminutive and spare form, with slender arms and shoulders, use with the greatest ease a bow he had been accustomed to, which one of the strongest of our soldiers could hardly bend.

perature of the air in Kandy was about 75°, I tried the temperature of several English soldiers.

No.	Age.	Years in India.	Temp. under tongue.
1	24	$0\frac{1}{2}$	98·75
2	99	2	98·5
3	27	$2\frac{1}{2}$	99°
4	36	16	99·25
5	28	4	99°
6	34	$0\frac{1}{2}$	99·5
7	25	1	100°
8	23	$0\frac{1}{2}$	101°
9	25	25	99°
10	23	$0\frac{1}{2}$	98°

The four first were in perfect health; the remaining six were in different stages of convalescence from intermittent fever. They were all cool, and had not breakfasted.

3.—*Of the Temperature of different kinds of Animals.*

My observations on the temperature of different kinds of animals have been made at intervals, as leisure and opportunity permitted, in England, Ceylon, and during a voyage to the latter island. Though pretty numerous, they are far from complete; the completion of the inquiry demands the co-operation of many observers.

1*st*, *Of the Temperature of the Mammalia.*

I may premise that in my experiments on the mammalia, with a few exceptions which will be particularly noticed, the temperature of each animal was ascertained by introducing a thermometer into the *rectum;* and I may extend the remark to the experiments on birds; and I may further premise, that, when the contrary is not noticed, the subject of the experiments appeared to be healthy.

Monkey (*Simia Aygula*).—At Colombo, on the 30th of May, air 86°, the temperature of this animal full grown in the axilla was 104½°; rectum, only 103½°.

At Amanapoora, in the Kandian country, on the 1st of June, air 73°, the temperature of another full grown monkey of the same kind, in the axilla, was 101°.

Pangolin (*Manis pentadactyla*).—At Colombo on the 4th of November, air 80°, the temperature of a young pangolin apparently sickly, was only 90°.

Bat.—In the neighbourhood of Colombo, on the 27th of September, air 82°, the temperature of one bat was 100°, and that of another 101°. The instant the animals were killed, the thermometer was introduced into the cavity of the abdomen. The species resemble the *Vespertilio peruviana* of Linnæus, but was much smaller.

V. vampirus.—At Colombo, on the 15th of October, air 70°, the temperature of this animal,

ascertained in the same way as the preceding, was 100°.

Squirrel (*Sciurus getulus?*)—At Colombo, on the 19th of October, air 81°, the temperature of this animal was 102°.

At the same place, on the 29th of September, air 84°, the temperature of a large black squirrel was 106°, in the thick fur of the groin.

Common Rat.—At Colombo, on the 8th of February, air 80°, the temperature of this animal was 102°.

Guinea-Pig.—Female half grown, 102° ; Male of about the same age 101°.* Chatham, 5th May.

Common Hare.—At Colombo, on 16th of June, air 80°, the temperature of this animal in the groin was 100°.

Ichneumon.—At Colombo, on the 4th of November, air 81°, the temperature of this animal was 103°.

Jungle-Cat.—At Colombo, on the 26th of February, air 80°, the temperature of a young animal of this species of Vivera was 99°.

Cur-Dog.—At Kandy, on the 29th of May, the temperature of an animal of this kind was 102·5, and of another 103·5, both nearly full-grown. The temperature of a bull-dog, in England, on the 27th December, was 102°.

Jackall.—At Colombo, on the 9th of April, air

* In abdomen, under liver, 103°.

84°, the temperature of two young jackalls was 101°.

Common Cat.—In London, on the 5th September, air 60°, the temperature of a full-grown cat was 101°, and in Kandy, on the 7th April, air 79°, the temperature of another was 102°.

Felix pardus.—At Colombo, on the 10th of February, air 81°, the temperature of a young fierce animal of this kind, about four months old, was 102°.

Horse.—At Kandy, on the 14th of June, air 80°, the temperature of a horse, of Arab descent, was 99·5.

Sheep.—In Scotland, I have observed the temperature of sheep in summer, to vary from 101° to 104°; at the Cape of Good Hope, in winter, air 67°, in six different instances I found the temperature of the African sheep to vary from 103° to 104°, and in Ceylon, in the neighbourhood of Colombo, air 78°, the temperature of one sheep was 104°, and that of another 105°.

Goat.—At Mount Lavinia, near Colombo, on the 27th December, air 78°, the temperature of a full-grown castrated goat was 103°, that of a female, about nine months old, 104°.

Ox.—At Edinburgh, in summer, the blood of an ox, flowing from the carotids, was 100°; in Kandy, on the 28th of May, air 80°, the temperature of an ox, ascertained in the same way, was 102°.

Elk.—At Mount Lavinia, on the 27th of Decem-

ber, air 78°, the temperature of a female elk was 103°.

Hog.—At Hanwille, in Doombera, on the 26th of November, air 75°, the temperature of the blood of a wild hog, flowing from the carotids, was 105°; at Mount Lavinia, air 80°, the temperature of two young domestic pigs was the same.

Elephant.—At Colombo, on the 22nd of September, air 80°, the temperature of a full-grown healthy elephant was 99·5. It was ascertained by placing a thermometer in a deep abscess in the back.*

Porpoise.—In latitude N. 8° 23′, on the 11th of March, air 72°, sea 74·75, the temperature of a porpoise was 100°. The animal was drawn into the ship alive. The instant it was killed I tried its temperature, by introducing a thermometer into the substance of its liver.

2.—*Of the Temperature of Birds.*

Falcon (Falco milvus?).—At Colombo, on the 24th of August, air 77·5, the temperature of this

* It was necessary to lay open the abscess to effect a cure. The operation was performed with an amputating knife; a deep incision, as was requisite, was made; the animal was kneeling down for the convenience of the operator—not tied—his keeper at his head. He did not flinch, but rather inclined towards the surgeon, uttering a low suppressed groan. He seemed conscious, that what was doing was intended for his good; no human being, similarly situated, could have behaved better. I think it right thus to record this instance, which I witnessed myself, of this animal's (may I call it) reflecting

bird was 99°; it had been shot a few hours, and its legs were broken.

Screech-Owl.—In London, in the autumn, air 60°, the temperature of this bird was 106°.

Jackdaw.—At Attapittia, in the Kandian country, on the 2nd of June, air 85°, the temperature of this bird, the instant it was shot, was 107·75.

Common Thrush.—In London, in the autumn, air 60°, the temperature of this bird was 109°.

Common Sparrow.—At Gompala, in the Kandian country, on the 3rd of June, air 80°, the temperature of this bird, the instant it was shot, was 108°.

Common Pigeon.—In London, in the autumn, air 60°, the temperature of this bird, confined in a cage, was 108°. At Mount Lavinia, on the 27th of December, air 78°, the temperature of two young pigeons, two weeks old, was 109·5; and of two, three weeks old, 109°.

Jungle-Fowl.—In Ceylon, near Tangalle, on the 20th of July, air 78°, the temperature of one jungle-hen, the instant it was shot, was 107·5; and in the afternoon of the same day, air 83°, the temperature of another was 108·5. The jungle-fowl of Ceylon, I may remark, more resembles the English pheasant than the barn-door fowl.

Common Fowl.—At Edinburgh, in the winter

power and conduct, which it is difficult to consider otherwise than rational. And so confident were the natives that he would behave as he did, that they never thought of tying him.

air 40°, the temperature of a full-grown hen was 108·5. At Mount Lavinia, in December, air 78°, the temperature of two hens was 110° (the one, half, the other, full-grown); that of a hen that had been sitting on her eggs three weeks 108°; that of an old cock 110°, and of two chickens, two months old, was 111°.

Guinea-Fowl.—At Mount Lavinia, at the same time, the temperature of a full-grown Guinea-hen was 110°.

Turkey.—At the same time, the temperature of a full-grown turkey-cock was 109°, that of two more of the same age 108·5; that of a full-grown hen 108°; and that of a young cock, two months old, was 109·5.

Procellaria æquinoctialis.—In latitude N. 2° 3′, on the 8th of August, air 79°, sea 81·5, the temperature of this bird was 103·5, and that of another 105·5.

P. Capensis.—In latitude S. 34° 1′, on the 11th of May, air 59°, sea 60°, the temperature of two birds of this kind was 105·5.

At Mount Lavinia, on the 27th December, when the temperature of the air was about 77°, the following observations were made on some of the inmates of the Governor's poultry-court.

Hen, half-grown - - -	110°
Cock, ditto - - -	111°
Chicken, two months old - -	111°
Another, of the same age - -	111°

Hen, full grown, laying eggs	110°
Hen, that had been sitting on her eggs about three weeks	108°
An old Malay cock	110°
A cock, ten weeks old, sickly	108°
Guinea fowl, full grown	110°
Turkey, cock, full grown	108°
Another, do.	108°
Another, do.	109°
Turkey, hen	108°
A young turkey, two months old	109·5
A full grown goose	107°
Another	107°
A goose, that had been sitting on her eggs nearly a month	106°
A young duck, twenty days old	110°
Another, five weeks old	110°
Another, two months old	110°
Another, of the same age	110°
Two full grown ducks	110°
A full grown drake	110°
Another, but younger	111°
A full grown teal, taken young, tamed	109·5
Another	108°

Young Snipe.—Colombo, Ceylon, September, air 83°, half fledged ; Thermometer, between thigh and body, 98°.

Another, less strong, 97°. It ate a few worms, seemed distressed, and lived only about 24 hours after.

Common Hen.—Colombo, November, *in recto*, 109°; after having been frightened by a Cobra Capello, not bitten, 111°.

At Trincomalie, in the same year, on the 9th of October, when the temperature of the air was 84°, that of five fowls, nearly full grown, was 107°. They had been in confinement in a small hamper two or three days. On the 18th October, in the same place, when the air was 82°, found the temperature of two full grown hens, not in confinement, also 107°.

Plover.—Near Minery, in Ceylon, in September, the temperature of a plover, just killed, was 105°.

Common Goose.—At Mount Lavinia, in December, air 78°, the temperature of two full-grown geese was 107°.

Common Duck. — At the same time and place, the temperature of a full-grown drake, of two full-grown ducks, and of four ducklings, from three to five weeks old, was 110°, and that of a young drake, full grown, 111°.

Peacock.—At Kornegalle, in July, air 83°, the temperature of a full-grown wild peacock was 105°; of a young pea-hen 108°.

Of the Temperature of the Amphibia.

Testudo Mydas.—In latitude N. 2° 27′, on the 19th of March, air 79·5, the temperature of a large turtle, caught a week before at Ascension, was 84°, *in recto*. Again, in latitude S. 2° 29′, on the 23d of March, air 80°, the temperature of the blood of

the animal flowing from the great vessels of the neck was 88·5. The turtle was sickly, and probably this heat was morbid. At Colombo, on the 4th of May, air 86°, the temperature of the blood of a turtle that had been caught the day before was 85°.

T. Geometrica.—At Cape Town, in May, air 61°, the temperature of this animal was 62·5. At Colombo, on the 3d of March, the temperature of a larger specimen was 87°, air 80°.

Rana Ventricosa.—At Kandy, on the 31st of May, air 80°, the temperature of two frogs of this kind, just brought from a damp shaded place, was 77°.

Common male Frog, in recto, 64°, immersed in water of 60°, in a small pond, near Edinburgh, June 23, air 60°.

Another frog, kept in shade out of water a quarter of an hour, in recto, 63°, air 62°. A decapitated frog of about the same size, kept there the same time, 62°, in recto.

A frog, just after it was taken, (it had hopped some way) after basking on wet turf, exposed to the sun, occasionally obscured by light clouds, in recto, 70°; water of pond 61°. The same frog, placed in the shade, where the thermometer was 62°, after a quarter of an hour, from 70° fell to 63°; and decapitated, and left another quarter of an hour, the air continuing at 62°, it fell to 60°, in recto.

Another frog, in a state of rest, taken from a shady place, surrounded with damp plants, in recto, 58°; the thermometer in the place from whence it was taken was also 58°.

Another frog, taken from the turf close to the pond, where it had been a short time basking, was 68°, *in recto;* the thermometer exposed in the same place amongst the grass was 65°.*

Iguana.—At Colombo, 4th September, air 82°, the temperature of this animal was 82·5.

Serpents.—At Colombo, on the 27th of August, air 81·5, the temperature of an elegant green snake, a species of Coluber, was 88·5, *in œsophago.* At the same place, on the 24th of August, air 82·5, the temperature of a small species of brown snake, another species of Coluber, was 84·5 *in abdomine.* On the 23d of September, air 83°, the temperature of different species of brown snakes, also belonging to the genus Coluber was 90°, *in œsophago.*

4.—*Of the Temperature of Fishes.*

Shark.—In latitude N. 8° 23′, on the 11th of March, 1816, air 71·75, sea 74·75, the temperature of a large female shark, just taken, and still alive, was 77° in the deep muscles near the tail.

Bonito.—In latitude S. 1° 14′, on the 29th of July, 1816, air 78°, sea 80·5, the temperature of the

* These frogs were all males (no females were found in the pond), they were full grown, and active; their testes large. In their stomachs were caterpillars and insects, and in one two small hard stones.

My observations seemed to shew that their temperature is variable, as might be expected, according to the activity of their respiration; and that, occasionally, it is at least three degrees higher than the moist body would be similarly circumstanced, independent of respiration.

heart of this fish, which lies very near the surface, was 82°, and of the deep-seated muscles 99°. These observations were made immediately after the fish was taken. I may remark, that the heart and gills of this fish were unusually large, and the latter of a dark red colour; further, that the muscles in general, which were very thick and powerful, were red like those of a porpoise, and that the bonito appears to be as fond of raising its head above the water as the porpoise itself: with these circumstances, probably, its extraordinary temperature is connected.

Common Trout.—Near Edinburgh, in the Spring, river 56°, the temperature of this fish was 58°.

Common Trout, Mount Cenis,* in a tank supplied with water flowing perennially from snow, I found at 40°, at three different seasons of the year, temperature of fish 42°, in excellent condition.

Trout, about half a pound weight, *in œsophago*, 58°; water of river, flowing from Loch Katrine into Loch Vanacher, 56°, July 5th.

Eel (*M. laterostris*), Chatham, May 14. Temperature in air 51°, *in œsophago*, 51°. The eel had been out of water several hours.

* The trout at Mount Cenis, in a lake about 5000 feet above the level of the sea, is always in season, which perhaps may be owing in part to the temperature of the deep water being probably always about 40°. In accordance with this, I am informed that the same is the case with the Char inhabiting the deep waters of Coniston Lake, in Lancashire, and also, it is said, of the trout of the Sorgue, at Vaucluse, in Provence—a stream which bursts out at the base of a mountain precipice, and the temperature of which probably varies very little throughout the year. On the 10th of April, 1830, I found it 54° at its source.

Flying Fish.—In latitude N. 6° 57′, on the 12th of March, air 77°, sea 77·5, the temperature of this fish, the instant it fell on the deck, was 78°.

5.—*Of the Temperature of Mollusca.*

Common Oyster.—On a rock about a quarter of a mile from the shore, off Mount Lavinia, where the water was about a foot deep, in December, the temperature of the common oyster was the same as that of the sea, viz. 82°.

Snail.—At Kandy, on the 11th of June, the temperature of one of a large species of snail that abounds in the woods of Ceylon, was 76°, and that of another, 76½°, after having been confined eight hours in a box, the temperature of which was 76¼°.

6.—*Of the Temperature of Crustacea.*

Cray-fish.—At Colombo, on the 16th of September, air 80°, the temperature of a large cray-fish that had been taken out of the sea two or three hours before, was 79°.

Crab.—In the neighbourhood of Kandy, on the 25th of March, the temperature of a small crab, of a species which is common in the mountain torrents of the interior, was the same as that of the water in which it lived, viz. 72°.

7.—*Of the Temperature of Insects.*

Scarabæus pilularius.—At Kandy, on the 30th of June, air 76°, the temperature of a beetle of this kind was 77°.

Glow-worm.—At Kandy, on the same day, in the morning, air 73°, the temperature of a large species of glowworm was 74°.*

Blatta Orientalis.—At Kandy, on the 28th of the same month, air 83°, the temperature of two

* The light of the glowworm does not appear to be combined with heat; and the same remark applies to the light of the fire-fly, and, I believe, of other luminous animals. On the luminous matter of the fire-fly, I made many experiments in 1816, at Colombo. I may mention here a few of the results. It is viscid, neither acid nor alkaline, has a strong and peculiar odour; affords no ammonia when decomposed by heat; it yields a brown empyreumatic oil. It continues to shine after it has been detached from the animal; in one instance, it shone after fourteen hours. The separation of the luminous part does not immediately prove fatal to the fly; I have known one live twelve, and even eighteen hours after. The luminous matter appears to be a secretion independent of the influence of external light: I kept the flies in darkness many days, supplying them with food, and they continued to shine. The life of the animal is more easily extinguished than its luminous property; thus, when plunged into carbonic acid, the luminous matter continued shining, after the animal was motionless; but its duration was short: the same was observed in hydrogen gas. The luminous matter detached, shines for a brief time in water, and even in water deprived of air by long boiling, in alcohol, in aqua ammoniæ, nitric acid, nitrous acid vapour, ammoniacal vapour, in carbonic acid, and in hydrogen. In the nitric acid, and the aqua ammoniæ, and their vapours, and in alcohol, the brightness of the light was increased on immersion; but it was of only of a few seconds' duration. In carbonic acid and hydrogen, it shone longer isolated than when forming a part of the insect. Sixteen fire-flies, confined

cockroaches was 75°; and on the 29th, found the temperature of two more the same, when the air was 74°.

Gryllus hæmatopus?—At the Cape of Good Hope, in May, air 62°, the temperature of two locusts was 72·5.

Apis ichneumonia?—At Kandy, on the 26th of June, air 75°, the temperature of a wasp was 75°.

Papillio Agamemnon.—At Kandy, on the 2d July, air 78°, the temperature of this butterfly, in abdomine, 80°, and in thorace, 81·5.

A large butterfly not unlike the preceding, in England, Nov. 4th, temperature of room 74°, thermometer placed between its wings and abdomen rose to 81°.

Scorpio afer.—At Kandy, on the 20th May, at noon, air 79°, the temperature of a large scorpion was 77·5.*

in a small vial afforded sufficient light to enable me with perfect ease to see the figures on my watch. The gland or organ by which the luminous matter is secreted, has a tubular arrangement, somewhat perhaps analogous to the electrical organs of the gymnotus and torpedo.

* The popular idea of the poison of the scorpion, is I believe, far from correct. From the few experiments I have made with this animal, I am induced to infer that its sting is less severe than that of our honey bee. A fowl wounded by a large scorpion in the breast, appeared to suffer little or no inconvenience; the punctured part was only very slightly red, and just perceptibly swollen. I have applied the peculiar fluid twice to the tongue; in one instance, it occasioned a slightly acrid sensation; and in the other, besides the sensation, it blistered the part slightly; the effect was very transitory; in a few hours it had ceased.

Even in Ceylon, the poetical idea, of the enraged scorpion sting-

Julus.—At Kandy, on the 18th of June, at noon, air 80°, the temperature of a julus was 78·5. It was of that species which emits a yellowish fluid, having the smell of iodine, and which colours the cuticle, not unlike iodine, but has no effect on polished steel.

8.—*Of the Temperature of Worms.*

The only worms, the temperature of which I have tried, were two kinds of leech, the *Hirudo sanguisuga*, and a species which in Ceylon is called the Jungle Leech, remarkable for living out of water in damp places. The temperature of both was the same as that of the water and air in which they were confined.

I may remark, generally, that in the few experiments I have made to ascertain the temperature of small animals of the lower classes, a very small thermometer was used in each instance, introduced through a small incision into the body.

IV.—*Conclusions and General Remarks.*

That the temperature of man increases in passing from a cold or even temperate climate, into one that is warm; that the temperature of the inhabi-

ing himself to death, is entertained; but I never witnessed such a result. On one occasion I saw the experiment of encircling a scorpion by fire, confidently made by an English gentleman, but without effect, no danger from the flames, no provocation could induce the animal to wound himself. Perhaps, occasionally, when extremely irritated, it may do so,—not knowing what he is doing.

tants of warm climates is permanently higher than those of mild; and that the temperature of different races of mankind, *cæteris paribus*, is very much alike, are conclusions which the preceding observations on man seem to warrant.

The first conclusion, I am aware, is not novel; but I do not know that it was ever drawn before, excepting from very scanty data.

The second conclusion, though conformable with the first, is I believe new; indeed it is contrary to a received opinion, that the temperature of man in warm climates is actually lower than in cold. The opinion alluded to, I conceive, arose partly from hypothetical views of the subject; and if I recollect rightly, it has been supported only by two or three observations recorded by Dr. Chalmers, in his History of South Carolina, which were made at a time when thermometrical experiments were not very common, and when the standard temperature of man was rated much too low. Further refutation of this opinion is perhaps unnecessary. The experiments I have made with all the care in my power, are so numerous, and their results are so consistent, that if I do not deceive myself, they put the question beyond the shadow of doubt, and fix as a fact, that if the standard temperature of man, in a temperate climate, be about 98° (which I believe is the greatest approximation to the truth) in a hot climate it will be higher, varying with atmospheric variations from 98½° to 101°

The third conclusion I believe to be perfectly accurate; I say *believe*, because it is difficult, if not impossible, to collect more than presumptive evidence on the subject. However, may not the evidence be considered sufficiently satisfactory, since the variation of the temperature of the different races I tried did not exceed, in degree, what may be witnessed amongst different individuals of a ship's company, all of one nation, or even amongst different members of the same family?—The similarity of temperature in different races of men is the more remarkable, since between several of them whose temperatures agreed, there was nothing in common but the air they breathed,—some feeding on animal food almost entirely, as the Vaida,—others chiefly on vegetable diet, as the priests of Boodho,—and others, as Europeans and Africans, on neither exclusively, but on a mixture of both.

Farther, that the temperature of birds, of all animals is the highest,—that of the mammalia next,—that of the amphibia, fishes, and certain insects next in degree,—and lowest of all, that of the mollusca, crustacea, and worms,—are conclusions, with few exceptions, that may be deduced from the preceding experiments on the temperature of animals in general.

Moreover, since in general, so far as experiment and observation have yet gone, there appears to be a decided connection between the quantity of oxygen consumed by an animal, and the animal's heat, is

there not good reason to consider the two in the relation of cause and effect?

If animal heat be owing to nervous energy, or any way connected with the nervous system, why, it may be asked, are birds so much hotter than the mammalia?—Why is the temperature of most quadrupeds higher than that of man?

Or, if it be owing to digestion, and secretion, and animal action, why is the temperature of the amphibia and of fishes so low, whose powers, in respect to these functions are so considerable?

Or, if it be connected with muscular energy, why are the animals, whose muscular powers are most remarkable (the animals belonging to all the lower classes), equally remarkable for the lowness of their temperature?

Or, lastly, if animal heat at all depend on peculiarities of structure or organization, why, it may be asked, is not the temperature of the amphibia elevated like that of birds,—the structure of the respiratory, and digestive, and secreting organs of the one class being so much like those of the other?

IX.

OBSERVATIONS ON THE EFFECT OF VIOLENT EXERCISE ON THE TEMPERATURE OF THE BODY.

Whilst I was in Ceylon, I made a few observations on the effect of severe exercise on the temperature of man, which, although less perfect and extended than I could wish, may be deserving of insertion here.

On the 5th of July, 1817, preparatory to setting out on a journey, I tried the temperature, both under the tongue, and in the axilla, of six natives belonging to a set of palankeen bearers, in good health and quite cool, at about seven o'clock in the morning, when the temperature of the air was 80°. The results were the following.—

No.	Age.	Therm. under Tongue.	Axilla.
1	35	98·5	-
2	30	99°	97°
3	35	98·5	96°
4	32	98·5	96°
5	35	99°	96°
6	33	100°	98°

At the end of the second day's journey, at four o'clock in the afternoon, immediately on halting, I tried the temperature of three of the bearers.

No.	Therm. under Tongue.	Axilla.
1	98°	98°
3	100°	98·5
4	98·5	98°

Before setting out, when at rest, the skin of these men was cool, it conveyed, indeed, to the touch of another, the sensation almost of coldness; now, in-instantly after rather severe exercise, when the temperature under the tongue was not raised, that of the axilla was increased and the surface of the body generally felt hot.

The next trial of temperature, under the influence of exercise, was made on the 26th July, on two of the same set of bearers, both about thirty years of age, both in good health, one stout and robust, the other rather slender. At ten A.M., at Rannè, in the Megam-pattoo, when the temperature of the air was 87°, after resting two hours, the temperature of the first man was, under the tongue, 99°, in the axilla 97°; and that of the second, under the tongue, was 98°, and in the axilla 97°.

A few minutes after ten o'clock, I set out for Tangalle. The sun was bright, the weather unusually hot, and the men, as if impatient, proceeded with more than ordinary diligence and speed. At noon,

when the air was 92°, I suddenly ordered them to stop, that I might ascertain their temperature. The first mentioned man, was one of the bearers carrying the palankeen, the other had been running by its side; both were sweating, the latter most profusely. The temperature of the first, under the tongue, was 98°, in the axilla 98·25; of the second, under the tongue, 99°; in the axilla 96·5.*

There are, on record, many instances of sudden death,—from drinking cold water, or from plunging into cold water, after exhausting fatigue, and when the body is commonly said to be heated. Probably in such cases, in conformity with the above observations, the temperature of the body has been actually reduced below its natural standard, taking the tongue as an index of the internal heat, and the fatal effect may, in part, be the consequence. This view of the effect was taken by the late Dr. Currie, who, in his Medical Reports, in the twelfth chapter, has collected many examples of the kind, and, amongst them, that interesting and impressive one of the catastrophe which befell the army of Alexander the Great, on

* I shall mention one observation more.—On the 10th September, 1817, after walking four miles over a very rugged road, in the interior of Ceylon, and sweating profusely; when the sensation of heat was greatest, a thermometer, placed under my tongue, was 98°, the temperature of the air at the time was 88°.

These observations are too few in number to admit of conclusions being drawn from them in a satisfactory manner. Few, however, as they are, the results are not uninteresting, especially those indicating a reduction of temperature under the tongue.

the banks of the river Oxus, where, according to Quintus Curtius, the loss of life was actually greater than had been experienced in any single battle. The circumstances of the case were, a forced march of forty-six miles, in hot weather, over a desert; excessive thirst and exhaustion; and, in this state, drinking large draughts of cold water.

X.

OBSERVATIONS ON THE TEMPERATURE OF THE INSANE.

In the Lunatic Asylum at Fort Clarence, attached to the General Military Hospital at Fort Pitt, Chatham, it has commonly been believed that the temperature of the insane is below the average temperature of man. This opinion has chiefly been founded on thermometrical observations, made many years ago by the late Mr. Tully, then surgeon to the forces, in medical charge. As the difference of temperature was, in many instances, stated to be of several degrees, varying from 92° to 97°, as ascertained under the tongue, taking 98° as the standard, I could not avoid entertaining doubts respecting the accuracy of the results; and this feeling induced me to institute some trials, for the purpose of endeavouring to arrive at some certainty, on a point not without interest, and importance, both theoretically, and in regard to the management of the insane. I shall give two sets of observations;—one set made in the depth of winter,—the other in the height of summer,—under circumstances, otherwise, as nearly like as possible;—viz. at the same time of the day, between 10 o'clock

and noon, at about the same interval between meals, the breakfast hour being 8, and the dinner 1, and after taking on each occasion about the same degree of exercise in the open air. The winter observations were made on the 17th January, 1838, when the temperature of the atmosphere was 30°; the summer observations, on the 4th of August following, when it was 68°; in the former instance the temperature of the room where the trials were instituted, was 43°; in the latter about the same as the open air.

TABLE OF OBSERVATIONS ON THE TEMPERATURE OF THE INSANE IN THE WINTER AND SUMMER OF 1838.

Name of Patient.	Age.	Species of Insanity.	Winter. State of health.	Temp. under T.	Summer. State of health.	Temp. under T.
Pte. Henry Dalton	55	Amentia	Pretty good	100	Pretty good	101
— Owen Eagar	63	Mania	good	100	good	101·5
Sjt. George Bayley	49	Mania	good	100	good	101
— H. Sutherland	31	Amentia	rather feeble	99	feeble	101
— John Roarke	41	Mania	good	98	good	101
Pte. Sam. Palmer	42	Amentia	good	99	good	100·5
— John Sharpe	50	Amentia	slightly ailing	102	good	99·5
— William Cane	62	Amentia	good	98	good	100·5
— Hugh Catlin	69	Amentia	feeble	100	feeble	101
— Ad. Walshatsky	51	Amentia	pretty good	99	moderate	100
— William Lyons	34	Amentia	pretty good	100	moderate	101
— Jacob Mundy	53	Amentia	good	99	good	101
— James Mullins	37	Mania	good	99	good	100
— C. T. Lewis	41	Mania		101	obscure disease of lungs	104·5
— James Davis	41	Amentia	pretty good	99	good	100
— George Roper	64	Mania	good	101	good	101
— John Fielding	45	Amentia	pretty good	100·5	moderate	101
— Luke Rielly	29	Amentia	indifferent	99	indifferent	99
— John Hennessy	57	Mania	good	99	good	100
— Michael Tudor	61	melancholia	good	100·5	good	101
— John Mayow	40	Amentia	good	100	good	101
— Ricd. Clements	61	Amentia	good	98	good	100
— Christ. Frisch	47	Amentia	good	101	good	101
— Elijah Walwark	27	Amentia	good	98	good	100

None of these patients were under coercion; the majority of them were chronic cases and incurable, labouring under erroneous ideas on certain subjects.

For the sake of comparison, I ascertained at the same time, both in winter and summer, the temperature of some of the individuals belonging to the establishment.

	Age.	State of health.	Temp. under tongue, 17 Jan.	Temp. 4th Aug.
1 Officer	48	good	98	99
2 Officer	50	delicate	100·5	—
3 Serjeant	53	good	100·5	100·5
4 Serjeant	51	rather infirm	101	101·5
5 Private	41	good	99	99
6 Private	27	good	98	—

From these observations it would appear that the temperature of the insane, as of persons not so afflicted, varies slightly in different individuals; that it is not lower in the insane, but rather higher; and that it varies slightly in summer and winter, being greatest in summer,—in accordance with the preceding observations, made on occasions of rapid transition from one climate to another.

The tolerance of cold and of heat by the insane, is a question totally different from the foregoing on their internal temperature. That they bear in most instances degrees of heat and cold, without complaining, which to sane persons would be disagreeable, is a well-established fact, however it may be accounted for. Probably neither heat, nor cold affects

their senses in the same degree as it does those in perfect health. Agreeably to this view, it may be remarked, that certain organic lesions occur in the insane, unaccompanied by the ordinary symptoms: thus, tubercles not only form in their lungs, but also tubercular excavations, often unattended with cough or difficulty of breathing, as if the parts had lost their sensibility. But, notwithstanding no warning is given of the mischief in progress, it runs its course, terminating in death as certainly and rapidly as if indicated by the common train of symptoms. The analogy may hold good as regards heat or cold in their influences on the constitution of the insane: whether felt or not, excess or deficiency of temperature may be as injurious to them, as to individuals possessed of the most delicate sense of them.

Incidentally, I would remark, that in this class of maladies, in which the ordinary symptoms of associated disease are so often latent, attention to the temperature, as well as to the pulse, may be of great use to the medical attendant. The pulse and the temperature may make him acquainted with the existence of disease, before not suspected, and may lead to inquiry of a minute kind, by which the exact species of the adventitious disease may be discovered. The case of C. T. Lewis, one of the patients in the lunatic asylum, whose temperature was ascertained and given in the preceding table, is a striking example of the use of such observations. On the 4th of August, the temperature under the tongue

was found to be 104·5, and his pulse rapid. This called attention to his state ; and, although he made no complaint,—his appetite good,—the functions, as far as was known, tolerably well performed, it was inferred he had obscure disease of lungs. He died in less than a month, namely, on the 26th of August. The fatal disease was pulmonary consumption, organically considered, in an aggravated form; but in relation to symptoms, most mild. There were ulcers of larynx without affection of voice;—most extensive disease of lungs, vomicæ and tubercles without cough; ulceration of intestines without diarrhœa; and disease of testes, vesiculæ seminales, and prostrate, of a severe kind, equally latent, excepting in the testicle, the hardness and enlargement of which, but without pain, were casually noticed.

XI.

OBSERVATIONS ON THE TEMPERATURE OF THE SHEEP IN WINTER AND IN SUMMER.

The majority of the foregoing observations, seem to show that the temperature of man, and birds, and quadrupeds, is somewhat higher in warm weather than in cool weather,—as in the hot atmosphere of the plains within the tropics, compared with the cool air of the mountains. The observations, however, made by our enterprising arctic travellers, during the depth of winter of intense cold, seem to prove that the animals inhabiting the extreme northern regions have a high temperature, perhaps higher than the same animals in a milder atmosphere. In illustration, I shall insert here the results of some trials made on quadrupeds, by inserting a thermometer several inches into the rectum before the death of the animals, commonly taken in traps, for which I am indebted to the Rev. G. Fisher.

1821.		Fahr.	Air at the Time.
Nov. 15, White Fox	-	106·75	+ 14°
Dec. 3, ,,	-	101·5	— 5°

		Faht.	Air at the Time.
1821.			
Dec. 3,	White Fox -	100°	— 3°
„ 11,	„ -	101·25	— 25°
„ 15,	„ -	99·75	— 15°
„ 17,	„ -	98°	— 10°
„ 19,	„ -	99·75	— 5°
1822.			
Jan. 3,	„ (female)	104·75	— 26°
„ 9,	A White Hare -	100°	— 25°
„ —,	White Fox (male)	100°	— 26°
„ 10,	„ -	100°	— 15°
„ 17,	„ -	106°	— 32°
„ 24,	„ -	103°	— 21°
„ —,	„ -	103°	— 21°
„ —,	„ -	102°	— 21°
„ 27,	„ -	101°	— 27°
Feb. 2,	A Wolf - -	105°	— 27°

The great difference of temperature of individuals of the same species, may have been owing to a difference in the length of time they had been entrapped.

Considering the highest temperature as nearest the natural temperature, the results are favourable to the notion above alluded to, that very severe cold may promote the production of animal heat. And, theoretically considered, this appears nowise improbable, much less impossible. Several circumstances may be supposed to conduce to it,—such as the very warm, or bad-conducting nature of the thick winter

clothing of the animals, in conjunction with extreme dryness of air,*—the great activity of the function of digestion,†—the greater activity of the function of respiration, and the consequent greater degree of aëration of the blood: As our fires burn with more intensity in winter than in summer, and give off more heat, so in animals a greater degree of heat may be produced under the circumstances alluded to, when most required.

With the hope of collecting some satisfactory information on this point, I have selected the sheep, as a fit subject for trial, and I shall now record the results which I obtained in winter, and spring, and summer.

On the night of the 22nd January, 1838, the wind changed to the north, and the temperature fell below the freezing point. It continued so, constantly, to the 26th of the same month; at night generally below 22°, and by day, at or below 29°. On the 26th, at noon, at Chatham, I made observations on

* This is very strongly indicated by a circumstance which I heard related by Mr. King, the enterprising and very intelligent medical officer, who accompanied Captain Sir George Back,—how, in the depth of winter, the blankets exposed to the air after being frozen, dried so completely, that, when brought near a fire, not the slightest appearance of vapour was observable.

‡ The journals of our arctic travellers offer very striking examples both of the accommodating power of the human stomach and the strength of digestion. The quantity of animal food consumed, when there is an ample supply, in the high northern regions, is marvellous. I have been informed by Dr. Richardson, that the daily allowance of meat to the servants of the Hudson's Bay Company, is eight pounds a-man.—Their food is entirely animal food.

three sheep, ewes, which, the preceding day, had been driven in from an adjoining marsh, and had been kept, during the night, in a cool out-house, where the temperature was below the freezing point. At the time of the trials the temperature of the open air was about 29°.

1.—Rectum - - -	104°
Blood flowing from jugular vein -	103°
„ from carotid artery -	105°
Base of brain - - -	104°
Near surface of cerebrum - -	103°
Left ventricle - - -	108°
Right ventricle (blood in it) - -	107°
Liver - - - -	107°

2.—Rectum - - -	104°
Mixed venous and arterial blood from great vessels of neck - -	105°
Brain at base - - -	106°
Surface of cerebrum - -	103°
Left ventricle - - -	108°
Right ventricle - - -	106°
Liver - - - -	106°

3.—Rectum - - -	104°
Mixed venous and arterial blood from great vessels of neck - -	105°
Base of brain - - -	107°
Towards surface of cerebrum -	106°

Left ventricle - - -	109°
Right ventricle (blood in it) - -	106°
Liver - - - -	106°

From ten to fifteen minutes elapsed between the examination of the brain and heart. The temperature of the rectum was tried during life. The head was cut off immediately after the flow of blood had ceased. The heart was not taken out till the skin and abdominal viscera had been removed.

In the first experiment, in which the difference of arterial and venous blood was tried, the trial was carefully made, and the result unobjectionable. The small bulb of the thermometer was introduced into the jugular vein, and the blood flowed freely over it; and, it was placed in the full stream of arterial blood, close to the wound in the carotid artery, after a ligature had been applied to the jugular.

The following observations were made on the 29th of January, between one and two o'clock, P. M., when the air was about 46°. It rained the preceding night; froze early in the morning; after eight, A. M. there was a continued thaw.

1.—Rectum - - -	105°
Venous blood, from jugular vein -	104°
Arterial from carotid artery -	106°
Right ventricle of heart - -	105°
Muscle of left ventricle, where thickest	108·5
Left ventricle - -	108°
Brain at base - - -	105°

Near surface of cerebrum - -	103°
Interior of globe of eye - -	100°
2.—Rectum - - -	105°
Vagina (mucous fluid flowing from it, period of æstus) - -	107°
Blood from jugular vein - -	106°
" carotid artery - -	107°
Base of brain - - -	106°
Near surface of cerebrum - -	105°
Right ventricle - - -	107°
Left ventricle - - -	108°
Interior of globe of eye - -	104°

It was my wish to have examined the heart whilst yet pulsating, to endeavour to ascertain if any heat is produced by the muscular contraction of the organ. The chest was opened within five minutes of the division of the great vessels of the neck ; but in both instances too late, the heart had ceased to contract.

The second sheep was in an irritable state ; the lungs abounded in granular tubercles, yet the animal was in excellent condition.*

The next observations were made on the 16th of March, when the temperature of the air was 51°.

* The prevalency of tubercles in the lungs of sheep is remarkable. In their granular state, they do not appear to affect materially the health or condition of the animal ; even a few small vomicæ seem to have very little effect. I believe it is precisely the same with the human species ; many facts which I have collected are in accordance with it. The analogy is instructive and well deserving of being kept in mind.

1.—Rectum - - -	106°
Jugular vein - - -	106°
Blood from carotid - -	106°
Right pleura* - - -	107°
2.—Rectum - - -	105°
Mixed venous and arterial blood from cervical vessels - -	106°
Both lungs - - -	107°
3.—Rectum - - -	105°
Mixed blood - - -	107°
Lung - - -	108°
4.—Rectum - - -	106°
Mixed blood - - -	107°
Lung - - - -	109°
Eye - - - -	104°

These sheep, as well as the two preceding, were ewes, about three years old. The sex of the following, the subjects of observation on the 23d of May, is not mentioned in my notes; the temperature of the air then was 63°.

* The temperature of the lung and pleura was found by introducing a thermometer through a small incision in an intercostal space, immediately after the division of the great vessels, whilst the heart was still acting.

On the 18th March, in the instance of a full-grown spaniel bitch, the temperature, in recto, was found to be 105°, and in the right pleura, the lung not wounded, 107°. 30 drops of hydrocyanic acid were now introduced into the pleura; in half a minute the bitch expired, convulsed. The chest was immediately opened, the heart was still acting, the temperature of the right ventricle was 104°, of the left ventricle 107°; both ventricles were distended with blood, which afterwards coagulated.

1.—Rectum - - -	105°
Mixed venous and arterial blood from neck - - -	107°
2.—Rectum - - -	105°
Mixed blood - - -	107°
3.—Rectum - - -	105°
Mixed blood - - -	107°

The next observations were made on the 21st August, between noon and two o'clock in the afternoon, when the temperature of the air was between 70° and 72°.

1.—Rectum - - -	105°
Mixed blood - - -	107°
2.—Rectum - - -	107°
Mixed blood - - -	108°
3.—Rectum - - -	105°
Blood - - - -	106°
4.—Rectum - - -	105·5
Blood - - -	107°
5.—Rectum - - -	104°
Blood - - -	105°
6.—Rectum - - -	105°
Blood - - -	106·5
7.—Rectum - - -	106°
Blood - - -	106°
8.—Rectum - - -	106°
Blood - - -	106·5

The two last were lambs about fourteen weeks old. The rest were ewes, about three years old, with the

exception of No. 3, which was a ram of about the same age. The testes of this animal were comparatively small, little more than half their autumnal size, yet they contained a good deal of spermatic fluid, abounding in the spermatozoa peculiar to the sheep.

These sheep had not been previously driven. The subjects of the following observations, made at one, P. M., on the 28th August, had been driven ten miles early the same morning, and had been exposed to the sun in the market, and in the butcher's yard. They were all ewes, and about three years old; the thermometer at the time was 79°.

1.—Rectum - - -	106°
Blood in jugular vein - -	106°
Stream of blood from carotid artery -	107°
2.—Rectum - - -	105°
Blood in jugular vein - -	106°
Ditto from carotid artery - -	108°
3.—Rectum - - -	105°
Blood in jugular vein - -	106°
Ditto from carotid artery - -	107°
4.—Rectum - - -	106°
Blood in jugular vein - -	105°
Ditto from carotid artery - -	107°

All these observations were made with the same thermometer, and consequently they are better fitted for comparison. But after comparing them, it does not appear to me that the results are sufficiently consis-

tent to admit of any positive conclusions being drawn from them. They seem to shew, that the temperature of the body is a little higher in the warm weather of summer than in the moderate degree of cold of our winter ; the heart perhaps being an exception. The subject demands further inquiry, especially as regards the effects of extreme cold.—I have brought forward such observations as I have made, with the hope that they may lead to others ; and I have given them, miscellaneous as they are, believing that they may have some little value in connexion with the theory of animal heat generally, which, to be true and permanent, must rest on a very extensive series of facts —or what is identical, be an induction from them.

XII.

ON THE TEMPERATURE OF SOME FISHES OF THE GENUS THYNNUS.

It is commonly believed and asserted by Naturalists that fishes generally, without exception, are cold-blooded; thus Linnæus, in his "Regnum Animale," characterizes them in relation to their blood by "Sanguine frigido;"* and Baron Cuvier, our latest and highest authority, not only admits it, but also undertakes to shew, that it must be so: thus, in the chapter of his "Histoire Naturelle des Poissons," on the general character and essential nature of fishes, he says, "ne respirant que par l'intermède de l'eau, c'est à dire, ne profitant pour rendre à leur sang les qualités arterielles que de la petite quantité d'oxigene contenu dans l'air mêle à l'eau, leur sang à du rester froid."†

It was many years ago, viz. in 1816, when on a voyage to Ceylon, that I first met with an exception to this universally received opinion. It occurred in the instance of the Bonito (Thynnus Pelamys, Cuv. and Valen.), the temperature of which, in the deep-

* Systema Nat. Lib. i. p. 12.

† Hist. Nat. des Poissons, Lib. i. p. 275.

seated muscles, in the thickest part of the fish, a little below the gills, I found to be 99°, when the surface of the sea, from which it had just before been taken, was 80·5, the difference being, the very remarkable one of eighteen degrees and half.*

This fact necessarily made a strong impression on my mind, on account of its singularity; at Malta, in 1833-4, when examining the heart and gills of the tunny, of the Mediterranean, (*thynnus vulgaris*, Cuv. and Valen.) my attention was recalled to it, on finding that the latter were supplied with nerves of unusual magnitude; that the heart, like that of the Bonito, was very powerful; that the fish equally, or even more abounded in blood; and that its muscles generally, like those of the Bonito, from the same cause, were of a dark-red colour. It immediately occurred to me that its temperature also might be high, analogous to that of the Bonito; and the result of careful inquiry amongst the fishermen of most experience in the tunny fishery, confirmed the conjecture. All who were asked, declared that the tunny is warm-blooded; and one of the most intelligent, when questioned as to the degree of heat, said, it was much the same, or little less than that of the blood of a pig, when flowing from the divided vessels of the neck on being killed; and this man was very competent to give an opinion on the subject, having been much employed in the fisheries on

* Vide p. 218.

the Sicilian coast, in which the viscera of the fish are considered the perquisite of the common fishermen, and are instantly taken out by them when the fish are caught.

From the tunny, I extended the inquiry to other fish of the same family; and learnt that the analogy holds good applied to all the species of the genus Thynnus, of Cuvier and Valenciennes, with which the Maltese fishermen are acquainted, viz. *T. brevipinnis*, *T. thunnina*, *T. alalonga*, and the two already mentioned; all of which abound in blood, have a powerful heart, red or reddish muscles, and also, as I have ascertained by particular examination, have their gills amply provided with large nerves. Not having been able to procure any of these fish alive, their exact temperature I have not been able to determine; but, from the reports of the fishermen, it would appear that the common Tunny is the warmest of the species, and in accordance with this, I have found its branchial nerves proportionally largest.

These nerves are represented in Plate XIII. of their natural size, in a young fish which was about three feet long, caught in the channel of Malta, in November. Immediately after quitting the cranium, they enter or swell out into ganglia of considerable size, more or less connected together, from whence five principal trunks proceed; the first four chiefly to the branchia, and are the respiratory nerves; the fifth, the lowest, to the œsophagus and stomach, and the

parts adjoining. In point of magnitude, these respiratory nerves almost rival the electrical nerves of the torpedo, and their origin is very similar, and their direction and associations, but with this marked difference between them, that the nerves of the Torpedo are entirely destitute of ganglia.

The respiratory nerves of the other species of Thynnus which I have examined, very much resemble the preceding. Those which are smallest belong to the Thynnus brevipinnis; and yet even in this fish, in comparison with fishes of other tribes, the respiratory nerves are large, and their ganglia considerable. This fish, perhaps, may be considered a link between the tunny family and the mackerel, on one side, and the pelamides, on the other; and the respiratory nerves of one of each of these genera which I have dissected, viz. of the Scomber pneumatophorus, and the Pelamys Sarda, have approached in magnitude those of the Thynnus last mentioned. What the temperature of these fish is, I have not had an opportunity of determining by trial; according to the statement of the fishermen I have consulted, they are cold-blooded. Reasoning from analogy, it may be conjectured that they will be found to be of somewhat higher temperature than other fishes less amply supplied with respiratory nerves.

As regards the rationale of the high temperature of the species of Thynnus, there appears to me less difficulty than in accounting for the electrical power exercised by the torpedo and gymnotus. The peculiar

function of the latter is performed by means of a particular organ, the most striking feature of which is a vast apparatus of nerves ; but this organization bears little or no analogy to any other natural, or to any artificial process hitherto known by which electricity is generated. Not so, the respiratory apparatus and associated organs in these fish of high temperature, they are essentially analogous to those of the warm-blooded animals of the other two classes, and hardly more different from the respiratory apparatus and associated organs of the mammalia, than those of the mammalia are from those of birds. The function of respiration in water is commonly considered similar to that in the atmosphere ; the same change, it is supposed, takes place in the blood in both instances ; the same change is ascertained to take place in the air of the water and of the atmosphere ; and increase of temperature in one case and the other is referred to the conversion of carbon into carbonic acid. The difficulty is not as regards the kind of effect, but the degree of that effect ; not an augmentation of one or two degrees above the temperature of the surrounding medium, but of many degrees. A consideration of some of the peculiarities of these fishes may help to diminish this difficulty, which I have little doubt will be removed entirely, when we are better acquainted with their structure, and better acquainted with all the sources of animal heat.

The most important peculiarities, are, I believe,

chiefly the following :—a large and powerful heart; abundance of blood; red particles in abundance; large gills, and a very large apparatus of branchial nerves. These, all, may be considered as concerned either directly or indirectly in the generation of heat. And, the circumstances for the preservation of heat are scarcely less remarkable,—as the manner in which the gills are defended by peculiarly strong opercula, abounding in fatty matter,—and the deep situation of the principal blood vessels, surrounded by thick muscles,—and in addition, the aorta, or great systemic arterial trunk, surrounded by the principal abdominal viscera, the kidneys, stomach, and liver.

Moreover, the habits of these fish, may in some measure contribute to their high temperature. They are frequently to be seen near the surface, and seem to have a delight in springing into the atmosphere. Aristotle, speaking of the tunny, says, "of all fish, it most enjoys warmth, and on that account swims near the surface and frequents sandy shores:"—I quote from the old Latin translation of Theodore Goza. "Thunni omnium maxime piscium gaudent tepore et ob eam rem arenam et littora adeunt; per summa etiam maris innatant quo teporis potiantur:"*—confounding (as I believe in this instance he has done) fondness for warmth, with the habits connected with its production.

In this enumeration of circumstances, which may

* De Hist. Animal, lib. viii. cap. xix.

contribute to the high temperature of these fish, both as regards its generation and preservation, I have intentionally been very general; in the present state of our knowledge I apprehend it would be useless to be more minute. It is not impossible that these fish may possess means for generating heat peculiar to themselves, and of which at present we have no adequate idea. And the situation of the kidneys, of which a considerable portion is even higher than the stomach and posterior to the gills, of large size, and abounding in blood, and well supplied with nerves, would lead to the conjecture, that these organs, in the function of imparting heat, may possibly act a part. Still, however, reflecting on the great proportional size of the branchial nerves, and guided by analogy, it is difficult to resist the conclusion, that they are not principally concerned in the performance of the function in question, and that these nerves, as means, are so very ample on account of the element inhabited, and the proportionally greater energy of function required to produce the same effect of elevation of temperature in the water and in the atmosphere. On any other view, it seems difficult to account for the branchial nerves of these fish being proportionally very much larger than the pulmonary nerves of the mammalia, and vastly larger than those of birds, of all animals the warmest.* Whether there is any immediate rela-

* The size of the pulmonary nerves of birds, and indeed of their

tion between the ganglia and the branchial nerves, and the generation of heat in these fishes is uncertain, and must necessarily remain so, as long as there is any doubt concerning the use of ganglia. The absence of ganglia on the principal nerves of the lungs of man,* and I believe of the mammalia

respiratory nerves generally, as far as my observations have extended, is so small as to be truly astonishing, compared with their very high temperature. And, on the hypothesis of nervous influence being essential to the production of animal heat through the agency of respiration, the necessary inference seems to be, that birds require less of this influence, than any other description of warm-blooded animals; which may be owing, perhaps, to their peculiarities of structure, both in relation to the diffused aërial means they possess of generating heat, and their peculiar means, in their covering of feathers, of preserving it; and, owing probably further, to their less expenditure of it, from the peculiarities of some of their principal secretions, especially those of the kidneys, skin, and lungs; their kidneys secreting an almost solid urine, their skin exhaling little moisture, and that not in sweat, but entirely by insensible perspiration, and their lungs though exhaling more, from the nature of the function they perform, yet less than might at first be supposed, part of the aqueous vapour contained in the air expired being, I believe, condensed before it enters the atmosphere, by the trachea, mouth and beak, which are always comparatively cool. And in accordance with this view, from the experiments of Messrs. Allen and Pepys, it appears, at least in one instance,—that the pigeon, one of the warmest of birds, consumes in relation to its bulk, even less oxygen, or produces less carbonic acid, than a quadruped, the guinea-pig, the temperature of which is several degrees lower.

* Haller, speaking of the great sympathetic nerve, which in man is so amply provided with ganglia, says, "In pectore notabiles ramos paucos edit, neque memini me alicujus momenti truncos vidisse qui ad nervum octavi paris accederunt, etsi ejusmodi nervi illustribus viris visi sunt." Element. Phys. iv. p. 260. And, according to Sir Charles Bell's views of the nervous system, none of the respiratory nerves are ganglionic nerves. Sir Edward Home (Phil.

generally, and of many birds, would lead to the inference, that the nerves in the instance under consideration, rather than the ganglia are chiefly instrumental, and that the latter are in some way subservient to the former,—but whether for giving sensation to the branchia, or for imparting extraordinary secreting power, so as to change the blood, or for some other purpose, remains to be ascertained.

In conclusion, I would beg to state some desiderata,—points, connected with the subject of these warm-blooded fishes, which may be considered as deserving of further inquiry.

1. The exact temperature of different species of the Tunny family.

2. The temperature of the blood, coming from and returning to the gills.

3. The specific gravity of the blood of these fishes; and the proportion of serum, fibrin, and of red particles which it contains.

4. The minute peculiarities of their structure.

5. The peculiarities of their habits.

It seems not improbable, that the investigation, if followed up, may not only throw light on the function

Trans. for 1825, p. 257.) has endeavoured to associate the production of animal heat directly with ganglia; but the instances he has adduced seem liable to great objection; and the fact, that the great sympathetic nerve in birds is comparatively little developed, even less than in some reptiles, and destitute of large central ganglia, such as the semilunar ganglion in the mammalia (at least in every instance I have carefully sought for them, I have been unsuccessful) seems fatal to his hypothesis.

of respiration in these fishes, and on the production of their high degree of temperature, but also that it may aid in elucidating some obscure parts of the theory of respiration in general, in connexion with that of animal heat,—and especially the question, whether any oxygen is absorbed by the blood in the lungs and carried into the circulation.

XIII.

OBSERVATIONS ON THE TEMPERATURE OF THE HUMAN BODY AFTER DEATH.

It has not come to my knowledge that any precise thermometrical observations have hitherto been made on the human body after death. I was induced to pay attention to the subject, from having, in one instance, whilst engaged in conducting a *post mortem* examination, found the deeply seated parts of a degree of temperature uncommonly elevated. As the inquiry needs as much accuracy as possible, I shall first relate the simple facts, and afterwards offer some reflections on them. It will be necessary to notice the cases in which the observations were made. This I shall do as briefly as is compatible with the objects in view. It may be premised, that they all occurred in Malta, in our military hospitals in Valetta, during a period of six months, namely, from July 28, to the following January, and that they were all of soldiers belonging to our regiments, serving in that island; and, further, that the bodies, in every instance, almost immediately after death, were

removed to a large, airy, and comparatively cool room, the old laboratory of the hospital of the knights, where they were to be examined, and where they were generally covered merely with a sheet, and placed on a table of wood.

1. Aged 23 years; was admitted into hospital on the 30th of July, 1828, labouring under symptoms (as it was supposed) of acute rheumatism, having severe pain, first in one shoulder, then in the other, followed by pain in the hips, attended with much pyrexia, and a very rapid pulse. He died on the 6th of August, a few minutes after seven A.M. As the weather at the time was hot, and dead animal matter putrefied rapidly, it was necessary to inspect the body as soon as possible. Accordingly, it was examined three hours and a half after death; when the temperature of the air of the room was 86°. The appearances most remarkable, discovered on dissection, were extensive collections of matter in the right shoulder, amongst the muscles on each side of the spine of the scapula, with sinuses extending to the axilla, and round the capsule of the shoulder joint; and, a lesion of the same kind, and as extensive, in the left hip, close to the head of the femur, spreading through the glutei muscles; and marks of incipient inflammation (as ecchymosis) in the right hip. The viscera were apparently sound. The right cavities of the heart, and the great vessels, were distended with liquid blood. The body was slender, but not emaciated. Its surface was warm;

the deeply seated parts felt very hot, imparting a disagreeable sensation, almost like that of burning, to the hand, in contact with them. The thorax was first opened, and afterwards the abdomen. After partial exposure of the surface of the contents of these cavities to the air for about ten minutes, a thermometer was procured. Placed under the left ventricle of the heart, it rose to 113°; and, under the liver, in contact with the lobulus Spigelii, to 112°.

2. Aged 27 years; a stout robust man, died suddenly in barracks, on the 6th of August, at about half-past eleven A.M. The body was examined at five P.M. There was a good deal of reddish fluid in the ventricles, and at the base of the brain. The lungs were distended with black liquid blood, some of which had passed into the branchia. There was very little blood in the cavities of the heart. The temperature of the air of the room was 86°. As soon as the cavities of the thorax and abdomen were opened, the bulb of a thermometer was in succession placed under the left ventricle of the heart, and under the lobulus Spigelii of the liver; in the former situation it rose to 108°; in the latter to 107°. About a quarter of an hour afterwards, introduced into the substance of the right lung, gorged with extravasated blood, it was 105°.

3. Aged 25 years; a large, stout, muscular man, was admitted into hospital on the 19th of August, with symptoms of continued fever. On the night of the 22nd, he became delirious; but only for

a short time. On the evening of the 24th he died suddenly and unexpectedly. The body was examined four hours and a half after death, during which time the temperature of the air of the room had been between 78° and 80°. No organic disease was discovered on dissection, excepting of a chronic kind, and slight, and not explanatory of the fatal event; indeed, the abdominal and thoracic viscera, and the brain, were unusually sound. Notwithstanding the body was large and fat, its surface was cold: but the deeply seated parts were warm. A thermometer introduced under the left side of the heart, just after the pericardium was opened, rose to 97°, and under the liver close to the vena portæ, to 101°. The brain had been previously examined; the cavities of the thorax and abdomen were opened about four hours after death. It may be remarked there was a great deal of blood in the cavities of the right side of the heart, and a small quantity in the left.

4. Aged 21 years; rather a large muscular man; was admitted into hospital on the 21st of August; and died on the 11th of September, of acute dysentery. The examination of the body was commenced about four hours and a half after death, and the thermometrical observations were made about half an hour later; the temperature of the air at the time was about 82°. The viscera in general were sound, excepting the large intestines, which exhibited the worst effects of dysentery. The trunk was warm; the extremities cold and rigid. A thermometer

placed under the lobulus Spigelii of the liver, rose to 103·5, and under the left ventricle of the heart to 103°.

5. Aged 23 years; a stout, muscular, well-made man; was admitted into hospital on the 9th of October, with symptoms of acute rheumatism, which were followed by those of remittent fever, but not very strongly marked, attended with a low degree of pyrexia, rather rapid respiration, and a pulse little accelerated. He died rather suddenly on the 16th of October. The body was examined fourteen hours after death; when the limbs were rigid and cold. The temperature of the air at the time was 69°. Some coagulable lymph was found adhering to the back part of the pleuræ; a corresponding portion of each lung was apparently hepatized; and, both the liver and spleen, and also the heart, were of a morbid degree of softness, and easily broken; more especially the two former, which were nearly of poultaceous consistence. A thermometer placed under the lobulus Spigelii of the liver, as soon as the cavity of the abdomen was opened, rose to 88°.

6. Aged 24 years; a large stout man; was admitted into hospital on the 12th of November, 1827, with symptoms of acute rheumatism; and died on the 4th of November, 1828, eighteen days after amputation of the thigh, on account of vast and complicated disease of the lower part of the limb involving the bone and knee joint, and twelve days after the ligature of the femoral artery, on account of

secondary hæmorrhage from the stump. The body was not emaciated. It was examined twelve hours after death; when the temperature of the air was 68°. The branches of the femoral vein were found plugged up with coagulable lymph; the trunk of the vein was lined with coagulable lymph, and there was pus, or, a purilord fluid, in the iliac vein. A thermometer placed under the lobulus Spigelii of the liver, rose to 94°, and under the heart to 93°.

7. Aged 23 years; a stout, well-made man; was admitted into hospital on the 9th of November; and died on the 29th of same month, of acute dysentery. The body was examined three hours after death, when the temperature of the air was 66°. A large abscess was discovered in the liver, of which there were no symptoms during life; and the colon throughout exhibited, in a high degree, the ulceration and disorganization peculiar to dysentery. A thermometer placed under the lobulus Spigelii of the liver, rose to 96°; in the substance of the right lobe (in which the abscess existed) to 98°, and the same under the left ventricle of the heart. About five minutes after, when placed in the posterior horn of one of the lateral ventricles of the brain, it fell tő 90°.

8. Aged 33 years; a tall, muscular, well-made man; was admitted into hospital on the 30th November; and died on the 1st of December, of apoplexy, with hemiplegia. The symptoms were most distinct and violent; a large quantity of blood

was abstracted, and energetic treatment employed. The body was examined two hours after death; during which time, the temperature of the room was between 60° and 64°. The cause of the symptoms was not discovered by careful dissection. No blood was found extravasated on the brain; not more fluid than is usual in the ventricles; no softening of any part of the organ; no tumour making pressure on it; and, not a greater degree of fulness of its vessels and redness of its membranes, than is frequently met with in cases in which the powers of sense and intellect have remained unimpaired to the last. A thermometer, placed under the lobulus Spigelii of the liver, rose to 86°, and under the right ventricle to 88°, immediately after the cavities were opened.

9. Aged 23 years; a tall thin man; was admitted into hospital on the 13th of September, and died on the 3d of December, of phthisis pulmonalis, of slow progress, and mild symptoms. During the last month, there was no cough, no dyspnœa, no uneasy feeling, a slight diarrhœa, gradual wasting, and loss of strength. The body was examined five hours after death; during which time, the temperature of the air varied from 59° to 64°. The lungs abounded in small grey firm tubercles, and in small curd-like tubercles, and contained a few vomicæ; the principal part of their substance was crepitous. The lymphatic glands of the thorax and abdomen, generally, were enlarged and softened in different degrees. There was an ulcer in the inner coat of the duode-

num; many ulcers on this coat of the jejunum; and the ileum and large intestines abounded in ulcers, many of them deep and large. A thermometer introduced into the substance of the liver rose to 90°: it indicated the same degree of heat placed under the lobulus Spigelii, and under the right ventricle of the heart, immediately after the thoracic and abdominal cavities had been opened. The skull-cap as soon as possible was removed, and immediately the membranes of the brain were divided by a deep incision, penetrating into the centre of the cerebrum, into which the thermometer being placed, it instantly fell to 82°.

10. Aged 28 years; rather small, but a well-made man; was admitted into hospital on the 23d of December, with symptoms of an obscure kind, seeming to indicate disease of the liver. He had occasional pain in the region of that organ, and also of shoulder; a rigor now and then; a rapid pulse; but little pyrexia, or loss of appetite, or wasting. He died on the 31st of January. The body was examined seventeen hours and a half after death, during which time the temperature of the room was from 58° to 60°. The liver, large in its dimensions, was found to be excavated in a singular manner, not unlike a tuberculated lung, in its advanced stage of suppuration, abounding in vomicæ. The excavations in the liver were extremely numerous and labyrinth-like, so irregular were they in form, and communicating with each other so irregularly. Their walls were in many

instances columnar, and the cavities intersected by columns containing blood-vessels, and by blood-vessels almost bare. Their contents were a viscid bilious purulent fluid ; and its yellow colour was apparently derived from bile discharged from the ulcerated ends of the branches of the biliary duct, indicated by bright dark-yellow spots on the surface of the excavations. In the situation of the vena portæ, there was a large sinus, which was ruptured by introducing the thermometer under the lobulus Spigelii ; it discharged a fluid similar to that described above. On minute examination, this sinus, lined with ragged coagulable lymph, and about thrice the size of the vena portæ, was found to be that vein itself vastly diseased : and the veins which terminate in the vena portæ were found terminating in it, empty, and their abrupt terminations in this sinus or sac closed with coagulable lymph. The hepatic artery, vein and duct (at least their trunks), were not diseased. The spleen was of usual size and appearance, but unusually compact and hard. A thermometer introduced into the centre of the cerebrum, so soon as the skull-cap was removed, rose to 70° ; about half an hour after when placed under the right ventricle of the heart, it rose to 82°, and the same under the right lobe of the liver.

These examples, which I have introduced in the order, in point of time, in which they occurred, are the only ones in which I tried the temperature of the body after death, in a warm climate. They

will probably be considered of very unequal interest, comparing those in which an unusually high temperature was noticed, with the majority in which the temperature was no higher than might have been expected.

It would be superfluous on this occasion to enter into any minute disquisition relative to the rationale of the very high temperature observed in the bodies Nos. 1 and 2, in relation to the different degrees of temperature noticed in the other bodies. The question will naturally be asked, Was the extraordinary temperature in one case, of 113°, and in the other of 108°, generated during life or after death? I have little hesitation in coming to the conclusion, that it was generated before death, and generated probably in the same way as the ordinary degree of animal heat experienced in health, or the extraordinary degree witnessed in febrile diseases. *A priori*, the effect of the heat-generating process, whatever it may be, can hardly be limited. In many birds, it raises the temperature of the body to 109°, when in perfect health, and in man to 101°, at least in the tropics, without deranging health; and it is easy to conceive, that, by increased activity or energy, it may exalt the temperature to the common febrile height, or to a height greatly exceeding that. But, destitute of life, there does not appear to be in the body any source of heat, any power of generating it, that we are aware of. Putrefaction had not taken place in these bodies; I believe I may say it had hardly obscurely com-

menced. Even if it had, and had made progress, and were it even at its greatest height of activity, it is doubtful if it would be equal to the production of the effect in question?

It may be matter of regret, that the temperature in the two first examples, was not ascertained before as well as after death. It is only known that the skin of No. 1, the day he died, was pungently hot —the expression I believe used by Dr. White, the zealous medical officer under whose care he was. As the weather at the time was oppressively hot, and the sensations of those in health, as well as of those labouring under disease, distressing more or less in relation to heat, less attention naturally would be paid to the degrees, and there would be the more difficulty in distinguishing them, and saying this is natural,—that morbid.

As the facts stand (and I trust I may venture to call these observations such), they may tend to put medical inquirers on their guard relative to the extreme limits of degree of animal temperature, and especially of the blood and deeply seated parts. If the temperature of a body, as in No. 1, be 113°, three hours and a half after death; and, if generated during life, before death it must have been still higher. Let us suppose some part of this individual inflamed, and observations made on the heat of the inflamed part; it is easy to conceive that its temperature might be found unusually high, — and greatly exceeding the average temperature of the

body in health, or in ordinary disease. If it were immediately concluded that this was an instance of the local generation of heat in the inflamed part,—it would be an assumption, and might be considered an error. And, have not observations of this kind been made and published, and adduced as arguments in reasoning on animal heat, as regards its theory?

Relative to the majority of the examples in which the temperature of the dead body was ascertained, I shall avoid making many reflections at present: I shall be well pleased if they induce others to continue an inquiry which may lead to interesting results, both as concerns physiology, and perhaps pathology.

The temperature of a part, perhaps, may prove a safe criterion, *cæteris paribus*, of the quantity of blood which passes through it in a given time. According to this test, it may be inferred, that the supply of blood to the brain, in the ordinary estimates of it, is over-rated.

May not the temperature of the deeply seated parts, after death, give some insight into the manner in which death has occurred, or help to elucidate its cause? In example No. 9, the temperature of the heart and liver was low. Was this connected with the state and function of the lungs, during the latter period of life, when much organic disease existed in them; when their functional action appeared to be feeble, and the quantity of heat generated small?

Additional Observations on the Temperature of the Body after Death.

The subject discloses, besides what have already been adverted to, other openings for curious and perhaps useful inquiry, especially in medical jurisprudence.

It may often be a question, how long a body has been dead. By attention to its temperature, particularly of the deep-seated parts, taking into consideration the circumstances affecting temperature, probably, in most instances, an answer may be given, approximating to the truth, and which may prove of considerable use in evidence.

As the preceding observations were all made in a comparatively warm climate, — it appeared to me desirable to extend them in this country,—particularly in relation to the application above mentioned. I shall now detail, such additional ones as I have had an opportunity of collecting, all of which have been made during the year 1838, in the General Hospital, at Fort Pitt, Chatham, on subjects similar to those on which the trials were instituted at Malta—being all soldiers. It may further be premised, that immediately after decease, the bodies were removed from the wards, and were kept in a room, covered merely with a sheet, in which the temperature was seldom more than 4° or 5° above that of the open air.

1.—Aged 26; died of pulmonary consumption

on the 17th January, at 8 A.M. and was examined twenty-eight hours after. Emaciation was not great. The lungs were extremely diseased, presenting cavities, numerous tubercles, induration and œdema, complicated with ulceration of trachea and large intestines. The quantity of blood generally was small. The temperature of the parts was tried in the order in which they are specified.

Between the hemispheres of the brain in contact with the corpus callosum, immediately on removing the calvaria -	46°
Under the left ventricle of heart (about an ounce of fluid in pericardium) -	52°
In the cavity of the right ventricle which contained a good deal of dark blood -	50½°
In that of left ventricle which was nearly empty - - -	51°
Under lobulus Spigelii - -	50°
In substance of liver - -	50°
In thigh, close to femoral artery, just before it dips under the sartorius muscle -	42°
Beneath the integuments of sole of foot -	39°

The thermometer during the whole time in the open air was at or below 30°; and in the morning early was as low as 13°.

2.—Aged 19; died of pericarditis on the 20th January, at 8 P.M. and was examined sixteen hours after. The pericardium was distended with 20 ounces of turbid serum, from which pus subsided on rest.

There was much wasting; little blood; 18 ounces of serum in the right pleura; 32 in the peritoneal cavity; œdema of limbs.

Longitudinal sinus, containing fluid blood	30°
Between the hemispheres in contact with the corpus callosum - -	51°
In lateral ventricle - -	51°
At base, under pons varolii, the brain in situ - - -	57°
In spinal canal, in which, and in ventricle, pretty much fluid - -	57°
In serum of right pleura - -	63°
In fluid of pericardium - -	65°
Under the left ventricle - -	67°
In fluid of abdomen - -	63°
Under œdematous integuments of thigh, near the artery as before - -	47°
Near the artery in calf of leg -	34°
Under integuments of dorsum of foot -	32°
of sole of foot -	31°

The atmospheric temperature during the whole time was low, at night below 20°, by day, never above 30°; the temperature of the room where the body was placed, was about 30°; when examined, it was about 40°.

3.—Aged 29; died of pulmonary consumption on the 26th January, and was examined eighteen hours after. There was great emaciation; little blood; there were the ordinary lesions in the lungs,

with empyema of left pleura, and ulceration of ileum and cœcum.

In longitudinal sinus (calvaria unusually thin)	55°
In lateral ventricle - -	55°
In upper part of spinal canal -	55°
Under integuments of sole of foot -	43°
of dorsum of foot -	44°
of thigh, close to artery	52°
In purulent fluid of left pleura (46 oz.)	64°
In right ventricle, moderately distended with blood, partly liquid, which afterwards coagulated - - -	63°
In left ventricle, empty - -	64°
In central substance of right lobe of liver	65°

The observations were made in succession, without interruption, in the space of five minutes. The temperature of the room the greater part of the time was about 40°.

4.—Aged 18; died of pulmonary consumption, on the 9th February; and was examined twenty-nine hours after. There was great emaciation; the lungs were very extensively diseased,—tuberculated, containing cavities, partially œdematous, partially hepatized; and the larynx, ileum and cœcum were ulcerated.

In third ventricle of brain - -	50°
Under medulla oblongata - -	50°
In spinal canal, at its upper part -	49°
Under lobulus Spigelii - -	56°

In fluid of pericardium (about two ounces) 57°
In right ventricle, which contained much blood - - - - 57°
In left, nearly empty - - 57°
Under integuments of thigh, close to femoral artery - - - 49°
Under integuments of sole of foot - 47°

The temperature of the room, during the time, was between 38° and 48°.

5. Aged 17 years; died the 20th of February, of peripneumonia; and was examined twelve hours and half after. The right lung was greatly condensed, and weighed three pounds and half; there was sub-emaciation and œdema of the lower extremities.

Between the hemispheres, on corpus callosum 57°
In lateral ventricles - - - 57°
In upper part of spinal canal - - 59°
Under the heart - - - 67°
Right ventricle, moderately distended with fluid blood, which afterwards coagulated 64°
Left ventricle, empty - - 68°

6. Aged 24 years; greatly emaciated; died on the 30th of March, of peritoneal inflammation, with ascites, variously complicated, and was examined fourteen hours after.

In longitudinal sinus - - 62°
In substance of right hemisphere - 66°

In lateral ventricles - -	64°
In upper part of spinal canal - -	64°
In the fluid in the abdomen (seven pounds)	73°
In right ventricle of heart - -	73°
In vena cava ascendens under liver, which weighed five pounds - -	77°

7. Aged 35 years; died 17th August, of malignant tumour in face, with great destruction of the surrounding parts, including a considerable portion of each jaw-bone. The body was extremely emaciated, and contained very little blood. It was examined five hours after death; the temperature of the air of the room at the time was 68°.

Between the hemispheres of cerebrum on corpus callosum - - -	86°
In fluid of lateral ventricles, which was in rather large quantity - .	87°
In cavity of pelvis - . -	90°
Under lobulus Spigelii of liver -	90°
Under heart, within the pericardium -	94°
Close to femoral artery, mid-way between hip and knee - - -	77°
Under integuments of sole of foot -	67°

8. Aged 40 years; died 18th August, of chronic dysentery, just after his return from the West Indies. The body was sub-emaciated; it contained pretty much blood and especially the abdominal

viscera. The examination was made four hours and half after death. The temperature of the air at the time was 68°.

Between the hemispheres on corpus callosum - - -	85°
In lateral ventricle, which contained but little fluid - - -	86°
In upper part of spinal canal - -	87°
Under lobulus Spigelii of liver -	97°
In cavity of pelvis - - -	97°
Under left ventricle - -	93°
In right ventricle, in which a good deal of frothy blood - - -	92°
In left ventricle, empty - -	95°
In middle of thigh, close to great vessels -	82°
Under integuments of sole of foot -	69°

9. Aged 22 years; died 19th August, (nineteen days after the amputation of his fore-arm, on account of disease of the bone), of complicated and severe organic disease,—chiefly latent,—as tubercles and vomicæ in the lungs,—abscess under the scapula, communicating with the shoulder joint, &c. The body was greatly emaciated, and contained very little blood. The examination was made eighteen hours after death; the temperature of the air at the time was 68°.

Between the hemispheres of the brain on the corpus callosum - -	68°
In lateral ventricle - - -	68°

At base of brain, where there was a good deal of fluid, and in the upper part of spinal canal - - -	68°
Under liver - - -	71°
Under heart, within the pericardium -	72°
In left ventricle, which was empty -	73°
In right, in which was some blood -	72°
In thigh, mid-way between knee and hip, close to femoral artery - -	65°
Under integuments of sole of foot -	64°

10. Aged 26 years; died 19th August, of pulmonary consumption. The body was sub-emaciated, and contained pretty much blood, which was not coagulated. The examination was sixteen hours after death; the temperature of the room was 72°.

Thermometer on corpus callosum -	67°
Under liver - - -	81°
Under heart - - -	81°
Under left ventricle - -	81°
In right ventricle - - -	80°
In thigh, close to femoral artery -	72°
Under integuments of sole of foot -	66°

These observations require little comment. Their application to the use of medical jurisprudence, in connexion with the question referred to, is obvious. Although limited in number, (and it would be tedious to extend them,) they may enable the inquirer, insti-

tuting similar thermometrical trials, and reasoning analogically, to arrive at a tolerably positive conclusion, in doubtful cases of death, as to the time which may have elapsed, between the fatal event and the post mortem examination.

Much judgment, however, and nice discrimination may be requisite on the part of the medical man, in appreciating the circumstances likely to modify temperature, so as to enable him, when called on for his opinion, to give one which will be satisfactory to the legal officers, and to himself, on reflection.

XIV.

AN ACCOUNT OF TWO CASES OF PNEUMATHORAX, WITH EXPERIMENTS AND OBSERVATIONS ON THE ABSORPTION OF AIR, BY SEROUS AND MUCOUS MEMBRANES.

The two following instances of Pneumathorax were communicated to the Royal Society, not as medical cases, but on account of the physiological questions connected with them, and the experiments of the same kind which they gave rise to. This alone could have entitled them to a place in the Philosophical Transactions, in which they were first published, in the volumes for 1823 and 1824; and it is this consideration which induces me to include them in the present work.

Case I.

Abraham Iredill, a private soldier, of the 7th Regiment of Foot, aged 30, was admitted into the General Military Hospital, at Fort Pitt, Chatham, on the 15th of January, 1823, labouring under phthisis pulmonalis, which proved fatal on the 11th of February.

The disease in this its last stage exhibited some peculiarities, the cause of which, referrible to pneumathorax, was not discovered until after death, for the case being considered hopeless, and the weather being very cold, the requisite examination of the chest was intentionally omitted.* The body was inspected fourteen hours after death. The right side of the chest exhibited a great degree of fulness; and it emitted, when struck, a hollow sound. On carefully opening the abdomen, the diaphragm was found protruding into the right hypochondrium, exhibiting a convex surface, almost conical, instead of concave; and it was tense and tympanitic. The right lobe of the liver was pressed into the epigastrium, and rested on a portion of the stomach and duodenum, and on a part of the transverse colon. Owing to the pressure of the liver, the pyloric portion of the stomach was removed from its natural situation to the left iliac region, where it rested on the upper part of the sigmoid flexure of the colon; and owing to the same pressure, the small intestines generally were driven downwards and more or less displaced.

The body having been immersed in a bath, a small opening was made with a scalpel under water, into that part of the right pleura best adapted by situation to allow of the escape of air. Air issued out in abundance, and with much force; 212 cubic

* At this time, the Stethoscope was not employed in the General Hospital; I first saw this excellent instrument, the following autumn in Edinburgh, and then commenced its use.

inches were collected in receivers, and about thirteen cubic inches escaped, making altogether the enormous volume of 225 cubic inches. The air collected was set aside for analysis, and the body having been replaced on the table, a portion of the ribs was removed from the right side to admit of the examination of the chest, the water which had rushed in to supply the place of the air having been carefully taken out and preserved.

The inner surface of the right pleura was covered with a thin layer of coagulable lymph. The right lung was exceedingly compressed ; it adhered closely to the upper part of the pericardium, and loosely to the posterior part of the chest (about the sixth and seventh ribs) by a few strong bands.

On inflating the lungs with a double bellows through an opening into the trachea, the right lung became much expanded, and air was found to pass freely from the lung into the pleura through an ulcerated opening in the upper part of the superior lobe.

The right lung was carefully dissected out. In the upper part of its superior lobe, a tubercular excavation or vomica was found, of the capacity of about four ounce-measures, communicating with the aspera arteria by a large bronchial tube, the ulcerated end of which terminated in the side of the excavation opposite to the openings by which the vomica communicated with the pleura.

On examining minutely the communication between the cavity of the chest and the lung, a kind of valvu-

lar structure was discovered, allowing of air being pumped into the pleura in the act of inspiration ; but not of its escape in expiration, to which no doubt was owing the great accumulation of air. Even at the risk of being tedious, I shall attempt to convey some idea of this structure. Between the false membrane of the vomica and the pleura, there was a small irregular sinus, not exceeding an inch in diameter, the sides of which, though not adhering, of course were in contact, or very nearly so. This sinus was the channel of communication, and contained the valvular structure alluded to. It opened into the cavity of the chest by a hole in the pleura pulmonalis, about the size of a crow-quill, and into the vomica by three smaller holes in the substance of the lung, not corresponding with the former, so that a probe could not be passed from one into the other in a straight line ; and, consequently, when the surfaces of the sinus were pressed together by the compression of the air in the pleura, in the act of expiration, the passage through which the air entered was closed, and its exit prevented.

A very brief notice of the other morbid appearances will suffice. Besides the vomica described, in the right lung, this viscus contained small tubercles, few in number in the inferior lobe, but abundant in the superior. The largest of them did not exceed in size a common pea, and the smallest were not larger than a mustard seed. The smallest were

translucent; the larger were of different degrees of opacity; all of them were solid; none had suppurated. The left lung was free from adhesion. Like the right it contained numerous small tubercles, which had made very little progress. The bronchia were redder than natural, as was also the lower part of the tracheá. Three ounces of serum were contained in the pericardium, and a larger quantity of fluid than usual in the ventricles of the brain. No air could be detected in the blood vessels, or in the cellular membrane of any part of the body.

To return to the contents of the right pleura; the water taken from this cavity (that which entered when the air was discharged) was turbid, as if from the admixture of pus. After resting twenty-four hours in tall glass jars, a white sediment formed, which, carefully separated by decantation, was about an ounce in quantity. It had the appearance of pus, and exhibited the properties of pus when examined by the most approved tests:—thus, it became viscid with a solution of muriate of ammonia; it was soluble in sulphuric acid, and precipitable by dilution with water; and it produced coloured rings when placed between two surfaces of glass, and held before a candle, according to the method recommended by Dr. Young. The decanted fluid, examined by a solution of corrosive sublimate, and by evaporation, was found to contain serum; and judging from the extract it afforded, it was about

11 ounces in quantity, half an ounce of the decanted water having yielded, when evaporated, 2·2 grains of dry residue.

The air collected from the pleura, had not the least fetor, nor indeed any smell. It extinguished flame, and was not inflammable. Examined by means of lime-water and phosphorus, (which was sublimed in it without effect) 100 parts of it, were found to consist of 8 carbonic acid gas, and 92 azotic gas.

Whence this air was derived became a question for consideration. Reflecting on the communication, discovered by dissection, between the pleura and the atmosphere through the medium of the lung, it seemed almost demonstrated, that the air was atmospheric air altered.

The next question was, how the alteration had taken place; what had become of the oxygen which had disappeared; whence the carbonic acid gas with which the azote was mixed?

To endeavour to learn how the oxygen had disappeared, the following experiment was instituted. The right pleura of a dog was inflated with atmospheric air, by means of a double bellows, and the incision through which the air was introduced was closed by a suture. At the end of forty-eight hours, the dog was killed. An hour after death, the pleura was punctured under water, and about 8 cubic inches of air were collected. This air, examined by means of lime-water and phosphorus,

was found to contain slight traces of carbonic acid gas, and to consist of 93 parts azotic gas, and 7 oxygen gas. The wound in the pleura was closed by coagulable lymph, and the pleura was found free from inflammation.

The result then of this experiment seems to show, that the oxygen was absorbed in greater proportion than the azote, and thus tends to account for the accumulation of the latter gas in the preceding case.

It may be said, that the experiment does not warrant the inference, that any azote was absorbed; and, consequently, that the expression "in greater proportion," is incorrect. The absorption of this gas is probable, however, though not demonstrated in the present instance, as Sir Astley Cooper has found, that common air introduced into the cellular membrane, and into the cavity of the thorax and abdomen of dogs, is, after a certain time, entirely removed by absorption.*

Relative to the source of the carbonic acid gas, it is easy to conceive that this gas was formed, or emitted in the air cells of the lungs, as in ordinary respiration; and that mixing with the air inspired, it was received into the pleura. If thus derived, and not from the surface of the pleura by secretion, it seems to follow, that it is less readily absorbable by the pleura, than oxygen. To endeavour to decide this point, the following experiment was made.

* Surgical and Physiological Essays, by John Abernethy, p. 55. London, 1793.

About 30 cubic inches of air, consisting of 80 parts common air, and 20 carbonic acid gas, were passed from a receiver into a bladder, furnished at one extremity with a stop-cock, and at the other with a small trochar; both air-tight. A small incision having been made through the integuments of the right side of the chest of a dog, the trochar was passed through the intercostal muscles into the pleura. The stilette was immediately drawn from the cannula into the bladder, and the air of the bladder instantly rushed into the pleura, and, on expiration, was in part forced back into the bladder. The exact quantity of air retained was not determined; it must have exceeded at least ten cubic inches. As speedily as possible the cannula was withdrawn, and the external wound carefully closed by suture. The health of the dog was very little impaired by the operation. Two hours after, when the animal appeared to be quite well, a similar experiment was made on the left side of the chest, and a mixture, consisting of 75 parts common air, and 25 carbonic acid gas, was introduced into the pleura. The operation had very little more effect than the former. At the end of twenty-four hours, the dog was killed, and immediately examined. About 3 cubic inches of air only were procured from the left pleura, which were found to consist of

18·3 carbonic acid gas.
78·3 azotic gas.
3·4 oxygen gas.

whilst the air admitted consisted of

20·0 carbonic acid gas
63·2 azotic gas
16·8 oxygen gas.

Thus apparently shewing, that during a sojourn of three days in the pleura, the oxygen had been absorbed in a greater degree than the carbonic acid gas; and the latter, in a greater degree than the azote. The result of the experiment on the left pleura was very similar; it afforded 10 cubic inches of gas, consisting of 25 carbonic acid, 70·6 azote, and 4·4 oxygen. The appearances on examining the wounds, were satisfactory: the cavity of the chest was free from inflammation, the lungs uninjured, and the cicatrix in the pleura only just perceptible.

The results of these experiments seem to warrant the conclusion, that in the preceding case, the carbonic acid gas found, was not derived from the surface of the pleura by secretion, or exhalation, but from the respired air, through the ulcerated opening. And, with this remark, I shall dismiss the case of pneumathorax,—the consideration of which, as a medical subject, would not be appropriate in this place.

The power exhibited by the pleura, in the preceding instances, of absorbing gases, and the manner in which it exercised that power, in a greater degree, on one air than on another, and that in no ratio to their solubility in water, appeared to me so interest-

ing and novel, that I was induced to prosecute the subject a little further. With the same apparatus, I made similar experiments on the admission of three other gases into the pleura of dogs, viz.,—hydrogen, nitrous oxide, and nitrous gas,—the results of which I shall briefly describe.

About twenty cubic inches of a mixture, consisting of 57·5 parts carbonic acid gas, and 42·5 hydrogen, were admitted into the left pleura of a dog, in the manner, and with the precaution, already noticed. The health of the animal was not apparently affected. At the end of two days, about thirty cubic inches of a mixture, consisting of 44·5 azote, and 55·5 nitrous gas, were passed into the right pleura. Immediately the dog's breathing became quick and short, but not laborious. It refused to eat, and expired in the evening, at the end of five hours from the time that the air was introduced. The next morning the body was examined. About six cubic inches of air were collected from the left pleura, consisting apparently of 12 carbonic acid, and 88 azote. After the removal of the carbonic acid gas, by lime-water, the residual gas extinguished flame, and was not itself the least inflammable; and hence the inference, that it was azote, or, at least, principally azote, as the presence of a small quantity of hydrogen might be concealed, and escape detection. From the right pleura, about five cubic inches of air were procured, which consisted of 6·9 nitrous gas, or air absorbable

by a solution of the green sulphate of iron, and of 93·1 azote. On opening the chest, the wounds in the pleura were found closed; the pleuræ were of natural appearance; the substance of the left lung was redder than usual, and that of the right was dark red, and it contained a good deal of blood and serum. The bronchia did not exhibit decided marks of inflammation. The right auricle, and ventricle, and venæ cavæ were distended with grumous blood; and the left auricle and ventricle, and aorta contained a good deal of liquid blood, which, as well as that of the venous system, had lost its peculiar tint, and had acquired a chocolate hue.

The obvious results of these two experiments, on the same dog, are,—1st, The absorption of the greater part of the carbonic acid gas and the whole of the hydrogen introduced into the pleura, and the appearance, *de novo*, of a considerable quantity of azote;—2nd, The death of the animal, in the space of five hours, from the time of admission of the nitrous gas and azote into the opposite pleura; the absorption of the greater part of the former gas, without inflammation of the membrane with which it was immediately in contact, and the production of a peculiar change in the blood.

Results so singular required to be narrowly scrutinized. I have twice repeated the experiment on the admission of hydrogen into the pleura of dogs, and in each instance, after death, I found that the hydro-

gen had disappeared, and that its place was supplied by a small quantity of azote.

Did the azote found, in these instances, exist in the pleura previous to the experiment?

A remark of Dr. Laennec would seem to countenance this notion. He says,—" M. Ribes assures me, that he has found, on opening the serous cavities of dogs, a small quantity of air constantly to escape." * On the contrary, in opposition to this, are the experiments of Haller and other accurate observers, recorded in the controversy which Hambergerus gave rise to, by reviving and maintaining the opinion of Galen, that air is contained between the lungs and pleura.†

In doubt between these contending authorities, with the desire of satisfying myself on the point, I have made some experiments on dogs, the results of which convince me, that in a healthy state, no air is contained in the pleura of this animal. When I opened under water the chests of dogs killed by drowning, not the smallest globule of air escaped; but when the right side of the chest was opened in the atmosphere, an appearance presented, at first favourable to the idea of a little air being contained

* A Treatise on the Diseases of the Chest, &c., translated from the French of R. F. H. Laennec, M.D., by John Forbes, M.D.—p. 208.

† Haller's Not. in Prælect. Boerh. D. C. vi.—Haller's Opuscula Anatomica de Resp. Gott. 1751, p. 91 and 345.—Marherr's Prælect. in Boer. Inst. vol. iii. p. 391.

in the left pleura, for the mediastinum was pressed from the left side towards the right (the body lying on the left side) evidently by air within the transparent membrane. This appearance, on examination, proved to be fallacious, for the air was found to be not in the left pleura, but in a cavity of the mediastinum communicating with the right pleura, and containing a lobule of the right lung, *dextri pulmonis additamentum*, as Haller calls it, who has noticed this structure in the mediastinum of the dog and many other animals, and pointed it out as one of the principal causes of the erroneous notion which he combated.*

Was the azote derived from the blood as an exhalation or secretion?

Facts might be advanced in favour of this idea. An exhalation or disengagement of azote appears to have taken place in the experiments of Messrs. Allen and Pepys, when oxygen, nearly pure, was respired.† In the inspection of dead bodies, air has frequently been found in the vessels and closed cavities; which is probably azote.‡

* Haller's Opuscula Anat. p. 44.

† Philos. Trans. for 1809.

‡ Vide Morgagni de Sed. et Caus. Morb. Ep. v.; and Trans. of a society for the improvement of Med. and Chir. Knowledge, vol. i. in which an interesting "case of Emphysema, not proceeding from local injury," is given by Dr. Baillie.

Notwithstanding the experiments detailed by Sir Everard Home, in his Croonian Lecture, published in the Phil. Trans. for 1818, I am induced to believe that the gas in question is azote, rather than carbonic acid;—because the alkali in the blood is not saturated with

It has been asserted lately, that air thus found is, in every instance, the consequence of putrefaction. But surely the accurate Morgagni was not so egregiously deceived. Many times, I have noticed air in the vessels of the pia mater, in bodies only a few hours dead; and very lately I detected some in the internal jugular vein of a body that had been dead only 18 hours, and free from every mark of incipient putrefaction; and I lay the more stress on this observation, because it was very carefully made, before any large vessel was divided through which air could gain admission. Further, air seems to pass pretty readily from the air-cells of the lungs into the pleura. Is not this proved by an experiment of Hales?* And an experiment which I have made,

carbonic acid; because the serum of the blood is capable of absorbing carbonic acid gas, rather more even than water, as I have ascertained by experiment; because, during the coagulation of blood spontaneously, and the coagulation of serum by heat, I have never observed carbonic acid gas to be disengaged, when the experiments were properly made in vessels to which air could not have access, as in tubes completely filled with blood or serum, and inverted in blood or Mercury; and lastly because I have not been able to procure carbonic acid gas from blood just drawn from the vessels, and still warm, when placed under a receiver and completely exhausted of air. I may here remark, that I have made two experiments on blood in vacuo, and in both with the same negative results. In one instance the arterial blood of an ox was employed; in the other, the blood of a man in health. In the former, eight ounces were used; in the latter, one ounce. In both instances, the blood remained perfectly tranquil, when the vacuum was as complete as could be made with a good air-pump, and of course did not exhibit the slightest traces of the disengagement of any air.

* Vide Stat. Essays i. 252.

and which I may briefly notice, seems to afford some proof of it. Immediately after death, before the muscles had lost their irritability, I inflated the lung of a dog under water, by means of a double bellows, through the trachea. Air, in exceedingly minute bubbles, escaped from the surface of the pleura covering one of the inferior lobes; and, on making gentle pressure with the fingers on any part of the inflated viscus, the same appearance presented itself.

These circumstances, which I have ventured to bring forward as somewhat favourable to the idea of the secretion or exhalation of azote, are still far from conclusive. After having given the subject all the attention in my power, I do not venture to draw a positive conclusion. I have thought it right to state what I have observed relative to a topic so interesting and obscure, and to notice such facts as seemed to bear more immediately on the question, in hope of exciting further inquiry, by which alone the true source of the azote apparently evolved in the preceding instance can be ascertained.

The effect of nitrous gas introduced into the pleura now requires consideration. I have made several trials farther with this gas. When admitted nearly pure into the pleura, it produced very serious symptoms; but did not prove fatal, provided the lung on the opposite side was free to act. The distressing symptoms usually subsided in about twelve hours, and then, on killing the animal, the greater

part of the nitrous gas was found to be absorbed; the pleura was free from inflammation, the substance of the lungs very slightly inflamed, and the blood exhibited a brownish hue. From these circumstances it may be conjectured that nitrous gas produces its deleterious effects, after it has been absorbed, either by acting on the blood immediately, or on the air-cells of the lungs and the blood conjointly, when converted into nitrous acid in the course of the pulmonary circulation.

On the admission of nitrous oxide into the pleura, I have made one experiment only. About 30 cubic inches of this gas, contaminated with 25 per cent. common air, were passed into the pleura of a dog. The animal exhibited no uneasy feeling, and immediately after appeared to be rather exhilarated. It continued apparently in good health for 24 hours, when it was killed. Five cubic inches of air were procured from the pleura, which consisted of 10 per cent. oxygen, and 90 azote, being quite deprived of nitrous oxide. The pleura and lung exhibited no unusual appearances which could be referred to the gas absorbed.

Mr. Abernethy, in his ingenious essay on the Functions of the Skin, has proved that that texture is possessed of a power of absorbing and exhaling certain gases, which it exercises according to laws peculiar to the animal economy.* The preceding experi-

* Surgical and Physiological Essays, Part 2, by John Abernethy.

ments seem to shew that the pleura is possessed of a similar power in respect to absorpton, and that in exerting this power, like the skin, it acts with greater energy and effect on one gas than on another. Whether the analogy will hold good as regards exhalation also, must be decided by future inquiry.*

Reflecting on the preceding experiments on the absorption of different kinds of air introduced into the pleura, it appeared probable that mucous membrane, like serous membrane and the skin, may possess the power of absorbing air. In relation to this view, I thought it worth while to examine the air contained in the antrum maxillare, and in the frontal sinus. I chose for the experiment the head of the sheep, in which these cavities are large, the openings by which they communicate with the atmosphere, small, and the membrane by which they are covered, an active secreting surface. I collected the air by perforating the cavities under water, about fifteen minutes after the death of the animal. In two different instances, the results of the examination of the air were the follow-

* When the experiments were made, which are detailed in the preceding pages, I was not acquainted with the ingenious and elaborate researches of M. Nysten on the injection of different kinds of gas into the blood-vessels and into the pleura, as described in his excellent work, "Recherches de Physiologie et de Chimie Pathologiques," published in 1817. The results I have obtained in most respects agree with his. Had he examined the residual air, and ascertained its composition, after the death of the animal experimented upon, the value of his researches, great as it is already, would have been much enhanced.

ing: the air from the antrum maxillare in one instance consisted of

4·3 carbonic acid.
13·0 oxygen.
82·7 azote.

From the frontal sinus, of

13·5 oxygen.
86·5 azote.

without any carbonic acid gas, the absence of which may have been owing to there being a good deal of mucus in the cavity; the mucus might have absorbed it.

In another instance the air from the antrum maxillare consisted of

4·2 carbonic acid
13·8 oxygen
82·0 azote;

From the frontal sinus, of

4·5 carbonic acid
9·5 oxygen
86·0 azote.

On the supposition that the air, previous to its entering these cavities, had undergone a partial change from respiration, the results described seem to indicate an absorption of oxygen.

Other facts might be adduced, which, like the preceding, though not conclusive, tend to support the idea, that mucous membranes are capable of absorbing air. Of this kind, I conceive, are the

results of the experiments of MM. Magendie and Chevreul, on the composition of the air contained in the human stomach and intestines;* and very recently I have met with a fact, the bearing of which, appears to me similar. In examining the body of a soldier, who had died of complicated disease, I found the cœcum exceedingly distended with air, and of a bright red colour, as if highly inflamed, whilst the ascending colon was unusually contracted. The air collected under water amounted to thirty-six cubic inches, and consisted of 11 carbonic acid, or air absorbable by lime-water, and of 89 chiefly azote, judging from its extinguishing flame, and not being itself inflammable. I had not the means of ascertaining if any traces of carburetted hydrogen were present.

The question, whether mucous membranes are capable of absorbing gases, I need not say, is one of great importance in relation to the theory of respiration, and on that account deserving of particular attention. The theory which in 1823 was most generally adopted, is recommended by its simplicity, but is not well supported by the analogies and facts of physiology, which seem to favour the doctrine of the absorption of oxygen into the blood, and the evolution of carbonic acid; and *that* perhaps, not in the air-cells of the lungs alone, but likewise

* Ann. de Chem. and Phys. ii. 292.

along the whole tract of the primæ viæ, and over the whole of the external surface of the body.*

Case II.

Patrick Calnon, of the 50th regiment, was admitted into the General Hospital at Fort Pitt, on the 9th of May, 1823, on his return from Jamaica, from whence he was sent home invalided on account of hæmoptysis, attributed to a severe fall on the left side of the chest, eighteen months previously. Before the accident, his health had been uninterruptedly good.

Until the 13th of May, his complaint exhibited nothing peculiar. Early on the morning of that day, after a violent fit of coughing, the symptoms of pneumathorax began to appear; and they conti-

* Many facts which, since the above was written, have been brought to light, especially by the researches of Dr. Edwards, might be adduced in corroboration of the remark in the text. In the winter of 1838, I examined the series of cells, filled with air, which occur in that beautiful osseous structure, the vertebral column of the wild swan. The contained air, I found composed of

83·3 azote
16·7 oxygen.

The examination was made two days after the bird had been shot: probably during life, the proportion of azote is greater. Probably, wherever air penetrates in birds, whether into the abdominal air-cavities, or the osseous ones, there is a continued absorption of oxygen going on, giving rise to several effects connected with the economy of this class of animals, as buoyancy, high aeration of blood, and high animal temperature.

nued to increase until the 21st. The most prominent symptoms were, a feeling of extreme tightness about the chest and abdomen; rapid and difficult respiration, between 30 and 40 in a minute; great anxiety of countenance and agitation of mind, accompanied with a small pulse of 130, cold sweat on the face and neck, and much prostration of strength. On examining the chest, the left side was found more protuberant, and in all its dimensions larger than the right. It was tense, and on percussion sounded remarkably hollow and tympanitic. The heart was felt beating on the right side, even under the mamilla.

Satisfied that the distension, and the distressing symptoms accompanying it, resulted from air in the pleura,—reflecting on the suddenness of the attack, the tolerable health the patient was in previously, and the now imminent danger of life,—I proposed in consultation the operation of paracentesis,—which was approved of, and with the ready assent of the suffering patient, immediately performed.

With a small trochar, attached to a flaccid bladder, I carefully perforated the left side of the chest, between the 8th and 9th rib, having previously divided the integuments with a scalpel. On withdrawing the stilette, a little air rushed out and was collected in the bladder, but not in the quantity that I expected; it did not exceed 5 cubic inches. On examination it was found to consist of azote, and a little carbonic acid.

Conceiving that the operation had failed in consequence of adhesions in the part of the pleura punctured; and encouraged by the composition of the air collected, and the slight relief which the patient experienced, a repetition of the operation was decided on, and performed the next day.

The chest now was perforated, just below the left papilla. On withdrawing the stilette into the bladder, a large quantity of air rushed out and distended the bladder; and on separating the bladder from the cannula (by cutting it off, after having secured the air in the former by a tight ligature) air from the chest, for several seconds continued to rush out with violence. When the rush of air had ceased, and it was found that air began to pass in on inspiration, the cannula was withdrawn, and the wound was closed by adhesive plaster.

The patient experienced sudden and great relief, exceeding his power to express.*

For about a month after the operation, the patient continued progressively to improve. On the 17th of June, he was as well, as when first admitted into hospital. His appetite was good, and his cough little troublesome. He could lie on the left side, which he was unable to do many months prior to the operation. Both wounds were healed. The

* The air collected in the bladder amounted to 25 cubic inches. Examined by means of lime-water and phosphorus, it was found to consist of 93 azote, and 7 carbonic acid; it had no offensive odour, or odour of any kind.

left side of the chest was diminished considerably in volume, and was much less tense and tympanitic. The heart, however, was still felt pulsating on the right side, and a fluctuation of fluid in air could be heard in the left side, on shaking, or suddenly moving the body.

The following week the symptoms were less favourable, and those of hydrothorax especially were more severe. When the patient attempted to lie on the right side, he was instantly seized with a fit of coughing; and when the body was shaken the sound of fluid fluctuating in air in the left pleura, was distinctly audible even at the distance of several yards.

As the patient's health was pretty good, it was deemed advisable, to repeat the operation of paracentesis and draw off the fluid, before the case became desperate.

Having, in cases of empyema, found some inconveniences attending the puncturing of the pleura, between an intercostal space, I was induced, in this instance, to follow the method described by Hippocrates, of perforating one of the ribs.* The fifth rib was selected; and having cut down upon it with a scalpel, just below the papilla of the breast, I bored through its substance with a carpenters' auger, and punctured the pleura with a small trochar, as nearly as possible of the same size as the auger. On withdrawing the stilette, it was followed by a

* Hippocrat. de intern. adfect. cap. xxiii.

stream of transparent fluid, fourteen ounces of which were allowed to escape; and, then, leaving the cannula in the perforation, it was closed with a cork, and secured by proper dressings.

Daily, during six weeks, more or less fluid was discharged through the opening in the rib, amounting, altogether, to more than twenty pints. At first, the fluid was transparent, of specific gravity 1021; it coagulated when heated, and contained some alkali in the state of sub-carbonate. In a few days pus appeared in it, and gradually increased in quantity until the 15th of July, when the discharge was almost entirely purulent; after which time, the proportion of pus and serum varied considerably,—one sometimes predominating,—sometimes the other. It may be deserving of notice, that at no time was I able to detect free carbonic acid in the fluid discharged.

Although, from the sound of fluctuation in the chest, air was evidently contained in the pleura, yet none escaped with the fluid during the first fortnight;—afterwards a considerable quantity was daily expelled. By means of the perforation in the rib, and using a flaccid bladder, furnished with a trochar, I could, at any time, collect this air for examination, with perfect ease.

The first portion collected (about twenty cubic inches) was on the 15th of July;—examined by means of lime-water and phosphorus, it was found to consist of

7·5 carbonic acid.
2.5 oxygen.
90·0 azote.

The second portion collected (about thirty-five cubic inches) was on the 20th July; it consisted of

6·0 carbonic acid.
5·5 oxygen.
88·5 azote.

The last portion collected (about forty cubic inches) was on the 29th of the same month; it consisted of

8·0 carbonic acid.
4·0 oxygen.
88·0 azote.

For some days after the operation of paracentesis, the health of the patient deteriorated; his appetite diminished; and he had feverish symptoms, but unattended with rigors. At the time, judging from the increasing proportion of pus which appeared in the fluid discharged, the pleura was probably undergoing inflammation, though the side affected was quite free from pain. Gradually these symptoms subsided; and, on the 15th July, he felt better than before the operation; he had less difficulty of breathing,—very little cough, and he could lie on either side. His improvement was progressive until the 23rd July;—afterwards his health again became worse, his appetite impaired and his spirits low; he had slight pyrexia, and a feeble pulse, varying in frequency from 90 to 120. This unfavourable change was

attended with the emission of a large quantity of air from the pleura, and with an alteration in the character of the fluid discharged,—which had become more purulent, of a greenish hue, and of an offensive odour. The patient expired suddenly on the 29th July.

The body was examined twelve hours after death. The cavity of the abdomen having been laid open, the diaphragm was found slightly protruding into the right hypochondrium, without displacement of any of the abdominal viscera.

The body having been immersed in a bath,—on puncturing the left pleura, between the first and second rib, air to the amount of one hundred and seventy cubic inches issued out, and was collected: it consisted of

16·0 carbonic acid.
1·5 oxygen.
82·5 azote.

The left pleura contained besides, about six ounces of pus,—so much having subsided from the water, which entered the chest to supply the place of the air.

The right pleura was quite free from disease. The right lung appeared to be healthy,—but, on minute examination, numerous granular translucent tubercles were detected, disseminated through its substance, and two small vomicæ were found in the upper part of its superior lobe. The heart was displaced: it was situated on the spine, inclining a little to the right side, and the position of the greater

part of the œsophagus was similar. The pericardium was firmly attached to the middle lobe of the right lung by a firm adventitious band. On exposing to view the left cavity of the chest, the inside of the pleura exhibited a surface of milk-white granular coagulable lymph, about two lines thick, equally diffused on the costal and the pulmonic side. Excepting the cicatrices externally in the skin, no traces could be detected of the first two operations, nor was there any mark of the last operation, exclusive of the small opening that had been carefully kept open, just large enough to admit the trochar, which had been daily introduced to draw off the fluid and allow the air to escape. On maceration of the rib, it may be remarked, a very narrow ring of bone was found exfoliating from the perforated part. The left lung was very much condensed, and so firmly confined by its thickened pleura, that it did not dilate when air was driven into it with some force by a double bellows attached to the trachea. This experiment was made with the lung under water, for the purpose of ascertaining, if any, and what kind, of communication, existed between the lung and the pleura, with a view to discover the origin of the air accumulated in this cavity. Two communications were thus detected; one in the inferior, the other in the superior lobe: the former was so exceedingly small that it could not be traced. The latter opening was sufficiently large to admit a surgeon's probe,—and its course was easily followed. It was found to

communicate obliquely with a ruptured opening in the side of a large bronchial tube, situated immediately under the pleura. No cavity could be detected there. The adjoining pulmonary substance appeared to be condensed from compression: the substance of the lung generally was in the same state; it contained a few minute tubercles; and no other traces of disease.

In the fatal case of Iredill, before described, pneumathorax took place in consequence of ulceration effecting a communication between the cavity of the pleura and a vomica in the lung. In this instance, the disease originated without the intervention of a vomica,—but, whether without ulceration, connected with the softening of a minute tubercle, is doubtful. When I first considered the case, I was disposed to attribute the communication to the rupture of a superficial bronchial tube, and to express surprise that accidents of this kind are not more common, taking into account the large number of bronchial tubes which lie immediately under the pleural covering of the lung,—the delicacy of the membrane, and the facility with which it and the bronchia are ruptured. The accuracy of this view appears to me now very questionable, inasmuch, as the delicate pleura has, in its healthy state, always the support of the parietes of the chest, and being constantly in contact with the costal pleura, the risk of its rupture may be supposed to be exceedingly small.

In a professional point of view, it might be an interesting, though not an easy task, to trace the different steps of the disease, of which I have given a brief history, from its commencement to its termination, and connect the symptoms, with the organic changes which occurred. As more appropriate to this place, I shall confine the few remarks I have farther to make, to the air procured from the pleura. The following table exhibits, at one view, the composition of the air collected from the chest at different times.

When Collected.	COMPOSITION.		
	Carbonic Acid.	Oxygen.	Azote.
May 21	7	-	93
July 15	7·5	2·5	93
,, 20	6	5·5	88·5
,, 29	8	4	88
,, 30	16	1·5	82·5

To what were these variations in composition which the table exhibits, owing? I cannot conceive that they depended entirely on the admission of variable quantities of atmospheric air by the external opening; because exceedingly little atmospheric air could enter through that channel, both from the great care taken to exclude it, and from the valvular

nature of the passage.* I believe we must look chiefly to the source of the air, and the absorbent power of the pleura for the explanation in question.

In this case, as in Iredill's, there is reason to suppose that the air accumulated in the chest was common air, more or less vitiated by respiration previously, and more or less altered by the process of absorption after entering the cavity of the pleura. Taking this view of the subject, the composition of the air, each time it was examined, is easily accounted for, excepting in the last instance, when the proportion of carbonic acid gas was found to be so large, after death. On what this depended it is not easy to say; it is matter for conjecture, and seems to require further investigation. Messrs. Allen and Pepys, after a forced expiration, found air from the lungs to contain as much as 9·5 per cent. carbonic acid gas;† and in the different instances that I have examined the air contained in the lungs a few hours after death, I have found the proportion of carbonic acid gas to vary from 8 to 12 per cent.; thus in a fatal case of empyema, the air

* The perforation being slightly oblique, the pleura costalis lined with coagulable lymph, closed the external aperture in the rib on expiration, and prevented completely the egress of air, even when the dressings were removed and the external aperture uncovered. When the trochar was introduced, the stilette was withdrawn during expiration, and the finger was applied to the mouth of the cannula during each inspiration.

† Phil. Trans. for 1808.

procured from one lung, which was sound, consisted of

8·3 carbonic acid
5·0 oxygen
86·7 azote:

that from the other lung, which was in part condensed, and as it were hepatized, of

12·5 carbonic acid
2·0 oxygen
85·5 azote:

whilst, in another case, in which one lung was sound and the other abounded in minute cavities full of pus,—air from the sound lung consisted of

12·2 carbonic acid
3·0 oxygen
84·8 azote.

Had the proportion of carbonic acid gas, in the instance under consideration, been within these limits, the explanation would have been attended with little difficulty; but, exceeding these limits, one is almost disposed to refer it to exhalation or secretion from the pleura. And if, at the same time, we suppose, that a similar exhalation took place into the bronchia, it might account for the sudden death.

In support of this idea of the secretion or exhalation of air into closed cavities, in addition to what has been already stated, I may notice the

two following instances, which have since come under my observation.

On the 23d of May, 1823, on examining the body of a soldier, who had died of chronic dysentery, complicated with an ulcer in the larynx, the cellular membrane in both mediastina was found vesicular and distended with air. The vesicles were burst under water, and a half cubic inch of air was collected, which was found to consist of

7 oxygen
4 carbonic acid
89 azote.

The surrounding parts were carefully examined; particularly the trachea, lungs and œsophagus, but no passage could be detected through which air could have entered the mediastina; nor could any air be forced into them by distending the lungs by means of a double bellows. It may be, that the oxygen found in the mixed airs, was extraneous and derived partly from common air adhering to the surface of the cellular membrane, and partly from air of the atmosphere penetrating through the delicate vesicles during the preparatory dissection, when they were exposed to its influence, for half an hour at least.

On the 2nd of June of the same year, in examining the body of a soldier, aged 36, who had died of tubercular consumption, I found air-vesicles on the surface of the lungs, similar to those de-

scribed by Dr. Baillie in his Morbid Anatomy;* and considered by him as resulting from the secretion of air, and not from the extravasation of air under the pleura, agreeably to the opinion advanced by Laennec.† The air contained in the vesicles in this instance, consisted of 5 parts of azote, and 1 part of carbonic acid. The quantity of air collected and examined, did not exceed 1-20th of a cubic inch; I could not detect in it, any traces of oxygen. In neither case, it may be remarked, were there any signs of incipient putrefaction; the weather was cool; and the examination was made a few hours after death.

* Fifth Edition, p. 80.

† Treatise on Diseases of the Chest, &c. p. 89.

XV.

ON THE EFFECTS OF THE SUN'S RAYS ON THE SKIN.

It is known to every one, that exposure to the sun's rays renders the skin brown; but I am not aware that this well known effect has hitherto been investigated with any minuteness, if at all, either in relation to the manner in which it is produced, or the part of the skin in which it takes place, or its exact cause, or its consequences.

The following observations were made with the desire of elucidating these points: they were collected whilst I was stationed in the Ionian Islands in 1826-27, and were originally published in the 3rd vol. of the Transactions of the Medico-Chirurgical Society of Edinburgh.

1. *Of the changes connected with the discolouring effect of the Sun's rays.*

For the purpose of ascertaining these changes, a portion of the back of the fore-arm, which had never before felt the sun's action, was exposed to

bright sunshine, in Corfu, during an hour and a-half, on the 29th July 1826, in the middle of the day, when the thermometer was at 78° in the shade. At the end of that time the skin was slightly painful, red, and hot. On the 1st August, the erythema was nearly in the same state; during the night the redness of the skin had been heightened, and the sensation of pain increased. On the 2d, there was very little alteration; a thermometer applied to the inflamed part rose to 96°, or 1° higher than when applied to the adjoining skin. On the 3d, desquamation had commenced at the circumference; there, where the cuticle had separated, the part was brownish-red, and not painful. In the middle, where the cuticle firmly adhered, the colour continued to be rose-red, and the pain continued, though in a less degree. This middle part, it may be remarked, in which the erythema was most durable, was most inflamed, the sun's rays having struck on it perpendicularly; whilst, on the circumference, from the rotundity of the arm, they impinged on it obliquely. On the 5th, desquamation was making progress; pain had ceased; the part was reddish-brown at the edges, but still red at the centre; the temperature of the part was not above that of the adjoining skin. On the 8th, the part was uniformly of a reddish-brown; desquamation was still taking place, the new cuticle separating almost as fast as it formed, not in continuous pieces, as in the first instance, when the old was detached, but in

small scales. On the 18th, the part was of a light-brown, with a very slight admixture of red, and its tendency to desquamation was very little greater than natural; in brief, it was in that state in which the skin is commonly said to be when tanned by the sun.

2. *Of the part of the Skin in which the discolouration takes place.*

Dr. Bostock, in his "Elements of Physiology," remarks, "It has not been ascertained upon which part of the integuments the sun acts, whether upon the epidermis, the corpus mucosum, or the cutis;" and he immediately adds, "but it is probably upon the epidermis, because we are informed that the tan of the skin is removed by blisters."

Were it a fact that the skin is rendered fair by blisters, the argument would be plausible,—I had almost said conclusive; but, as it is well known that blisters themselves render the skin brown, this argument can hardly be received.

From the observations which I have made, and from analogical reasoning, I am disposed to believe that the discoloration takes place beneath the cuticle, and that the seat of it principally is the surface of the cutis.

1*st*, As the sun's rays bleach hair, and as there is a considerable analogy between the hair and the

epidermis, its effect on the latter, it might be expected, would be similar.*

2*dly*, I have carefully examined the cuticle detached in consequence of inflammation from insolation, and I have not found it tanned in the slightest degree.

3*dly*, Are not the phenomena described in the preceding section, relative to the immediate effects and consequences of exposure to the sun's rays, almost sufficient to convince one that the cutis is the true seat of the discoloration? Were the epidermis the seat of it, it ought to be immediately discoloured by the sun's rays, which it is not; and when the epidermis separates, the skin should be fair; but the reverse of this is the case,—not till it separates does the skin lose its bright rose-red hue; and not till after several successive desquamations

* It is worthy of remark that the effect of the sun's action on the human hair is quite different from its effect on the skin,—bleaching the one, as much as it darkens the other; tending to bring both to a state as regards hue, best adapted, probably, for defence, such as occur in the Arabian horse, the finest breed of which have a white coat on a black ground, they are in fact Negro-horses with white hair. Every observant traveller must have witnessed instances in illustration, of what has just been observed. The most striking example of the kind which I recollect, is afforded by the female inhabitants of the border town of Itri, on the confines of the Neapolitan and Roman States, where the women are in the habit of exposing themselves, their heads uncovered in the open air; and in consequence there is a striking contrast between the dark sun-burnt countenances, and their comparatively light hair, which, variously discolored by the action of the sun's rays, and generally much neglected, imparts to them an appearance of extreme wildness.

is the tan of the skin well impressed and established, and many months elapse before it disappears.*

4thly, I have examined, with some attention, the cuticle of the Negro, of people of colour, and of Europeans who have become dark-brown from exposure to the sun's rays within the tropics. In each instance, when detached, it has appeared much less coloured than the skin; and, when minutely inspected, it has been found to owe its colour to colouring matter attached to it, detached from the cutis.

Lastly, I have preferred assigning the surface of the cutis as the seat of the discoloration (supposing it to be proved that the cuticle is not), rather than the rete mucosum or corpus mucosum of authors, as the very existence of such a texture is problematical. From the experiments which I have made on moles, and the coloured areola of the mamilla of fair persons, and on the skin of the Negro, I am disposed to believe that the colour, in all these instances, is owing to a colouring matter deposited in minute particles or filaments, on the surface of the cutis, as a secretion analogous, in its chemical properties, to the pigmentum nigrum of the eye.†

* The discoloration produced in August, by exposure of the skin for an hour and a half, at the expiration of seventeen months, was just visible. I may add, that I have found it to continue much longer on a part always covered, as the arm, than on the back of the hand, which has been covered only in the open air.

† I find that the colouring matter of the skin of the Negro, and the pigmentum nigrum of the eye, are acted on very similarly by

In the skin of the white, even in the parts discoloured, as in the areola already mentioned, I have not been able to discover any traces of a corpus mucosum, when the cuticle has been separated by means of immersion in the sulphurous acid. I have found the brown colouring matter, as before noticed, impregnating the surface of the cutis, and to be separated with difficulty by scraping it. In the case of the Negro, the colouring matter is deposited more thickly, and more in the form of a membrane; yet I have not been able to detach it as a membrane, and only in very minute portions, and that by scraping, when the cuticle has been raised and separated with as little inflammation as possible. The evidence in favour of the existence of a corpus mucosum, obtained either by maceration of the integuments, or by the application of blisters, appears to be very doubtful. By the first process, a gelatinous or mucus-like surface may be formed; by the second, a false membrane may be produced by the effusion of coagulable lymph, exactly resembling a corpus mucosum. I do not make these remarks hypothetically, but from experience,—from obser-

the three mineral acids, and a solution of potash, when heated, and by the sulphurous acid. By the former, both are dissolved; by the sulphurous acid, they are rendered of a light brown colour. They are not dissolved by these acids, or by the alkali, when cold; nor is their colour changed by a solution of chlorine in water,—contrary to what is commonly asserted of the colouring matter of the skin of the Negro. Both bear a high degree of temperature, apparently without change, viz. that nearly of a dull red heat.

ving the effects of blisters on parts of the skin in which there have been moles; on the areola of the mammilla; and on the skin of the Negro. In all these examples, the effects generally are very similar. If the blister is mild, the cuticle is simply raised; in the instance of the Negro, with a very little colouring matter adhering to it. When severe and long continued, not only is the cuticle raised by serum effused, but also by coagulable lymph, to which is attached colouring matter, and which may easily be mistaken for a coloured rete mucosum, and which is easily separated as a continuous membrane. When severe inflammation and suppuration is excited, the colouring matter either comes away spontaneously, or is most easily detached.* It appears to be most firmly connected with the cutis in the instance of moles, next in that of the brown areola of the nipple, and least in that of the black skin of the Negro. The part, in healing, when covered with the first formed cuticle, is red; it soon becomes brownish, but a considerable time elapses before it acquires its former intensity of colour. In the instance of the Negro, in which I have watched its progress, the secretion of colouring matter began at the edges, and spread towards the centre; and then, after a few days, spots of black appeared in the middle, which enlarged till the whole area was

* Thus it may be obtained in large pieces, very much resembling a membrane; but the connecting medium, I suspect, is coagulable lymph.

coloured. When a part not discoloured is blistered, in healing, it passes from red to brown ; and it is often a long time before the part regains its healthy hue ; generally, I believe, the fairer the skin, the less it is made brown by a blister, and the sooner it recovers its original whiteness ; and I believe the hotter the climate and season, so much the more slowly it regains it.

3.—*Of the Cause of the Change of Colour, and of the manner in which it operates.*

My experiments relative to the cause of the change of colour produced in the skin by the sun's rays, are not so satisfactory as I could wish. They tend, however, rather to prove that the effect is produced solely by the undecompounded rays. I have exposed, for more than two hours, and that repeatedly, the delicate skin of the under part of the fore-arm to the solar spectrum ; and I have concentrated the differently coloured rays of the spectrum, by means of a lens on the skin, but without occasioning either erythema or discoloration.

Relative to the manner in which the effect is produced, is it immediate and direct ; or mediate and indirect ? In other words, is it the simple effect of the sun's rays impinging on the skin ; or the effect of the inflammation which they occasion ; or do the sun's rays act in both ways ?

That they act powerfully indirectly in producing

discoloration, by exciting inflammation, the facts already mentioned are, it appears to me, sufficient proof. Indeed, whatever cause excites inflammation or irritation of the skin, seems to have an analogous effect, and to discolour it. Erysipelas, erythema, most of the exanthemata, burns, ulcers, excoriations, all occasion this effect (and, I believe, cold even is not an exception*), in different degrees, and very much in proportion to the intensity of the preceding inflammation, but whether exactly in that ratio it is difficult to determine. And we witness something of the same kind in mucous membranes; at least, I have observed that the cicatrices of old ulcers of the intestines are always discoloured, and either grey, blue, or almost black, apparently according to the degree of severity of the local disease which preceded them.

I have just said it is difficult to determine if the effect of discoloration be exactly proportioned to the inflammatory effect. There are circumstances in favour of its not being so. The erythema pro-

* Is not the dark colour of the inhabitants of the Arctic regions as much owing to the inflammatory or irritating effect of the extreme cold of winter, as to the scorching influence of the continued sunshine of summer?

Milton's highly poetical expression, the burning "frore" which he uses in his description of the damned regions, is in accordance with accurate observation of the agency of intense cold, in many respects so analogous to that of great heat:

"The parching air
Burns frore, and cold performs the effect of fire."

duced by strong acetic acid, and the vesication occasioned by the leaf of the common walnut tree, are followed by discoloration unusually dark and durable. Nor are there facts wanting which indicate that the change of colour may take place without inflammation, and go on increasing in intensity gradually, from continued exposure to the sun, or even too bright light, without inflammation having been once produced. I remember an instance demonstrative of this, in the person of an excellent and most amiable young officer (now no more), a case of tubercular phthisis, complicated with other organic disease, who, in hope of deriving benefit from sailing, and sea air, was taken from his room, where he had been confined many months, and conveyed on board ship, where he was placed under a convenient covering constructed on deck, sheltered always from the direct rays of the sun, but exposed to the bright light of the summer sky of the Mediterranean. In a short time, thus situated, he lost the pallid hue of the sick-chamber, and became almost as deeply tanned as a native of southern Europe; and I was particular in ascertaining that the change had not been preceded by the slightest erythema, or any sensible desquamation. I may mention, in confirmation, the result of exposing, a second time, to the sun's rays, the part tanned as in the experiment first related.

On the 18th August, the part first acted on was exposed for two hours, between 10 and 12 o'clock,

when the sky was unclouded, and the temperature, in the shade, about 80°. Immediately after this exposure, the tanned part was browner than before, and the adjoining white part, now exposed for the first time, was slightly red. On the 19th, the tanned part was distinctly browner and redder, a very little warmer than natural, and very slightly tender. The adjoining part was florid red, slightly painful, and hot. On the 23d, the tanned part was merely brown, a shade darker than before, while the adjoining part was undergoing desquamation, and beginning to lose its vivid inflammatory hue. And, farther, in confirmation, I may relate, that I have been at some pains to learn from natives of the Ionian Islands, especially of the lower classes, who are very much in the open air, what effect they have experienced from the sun's action. The result of my inquiry is, that very few of them have ever experienced the blistering or scorching effect of the sun; and when they have experienced it, it has commonly been on a part of the body not accustomed to be exposed to light; and on some occasion of unusual exposure, as that of bathing in the open sea. From all which, may it not be inferred, that the sun acts both indirectly, by the medium of inflammation, in changing the colour of the skin, and directly, without the intervention of inflammation, in producing the same effect, or in heightening it when produced?

Lastly,—Of the consequences of the Discoloration of the Skin.

Sir Everard Home, in an interesting paper published in the Philosophical Transactions for 1821, has proved, that, when the skin is painted black, it is defended from the scorching effect of the sun's rays; and, he thence infers, that the dark rete mucosum of the Negro possesses the same protecting power.

I have made experiments similar to those of Sir Everard Home, and have modified them, and all of them with the same results. All the opaque colours of which I made trial, applied to the skin, whether red, orange, blue, or green, have afforded protection from the scorching influence of the sun's rays, equal to that afforded by black.*

But though I have confirmed the experimental results of Sir Everard Home, it appeared to me, when reflecting on the subject, that his inference was not so well established as at first view might be conceived. It is founded on analogy, and that analogy not perfect; there being this difference between the skin of a white person painted, and of a negro with a black skin; that, in the one instance, the

* Considering the effect mentioned above, the usage of the ancient Britons of painting their bodies, may be referred to a purpose of utility, as well as of barbarous ornament,—the paint, with which they bedaubed themselves, must have answered, in part, in place of clothing.

black surface is laid on the semitransparent cuticle, whilst, in the other, it is situated under the cuticle, and on the surface of the cutis. In the one instance, the extinguishing medium is external to the insensible covering of the body; in the other, it is in contact with the sensitive surface, and may be considered as a part of it. Circumstances, too, relative to the very great penetrating power of the sun's rays, have had rather a similar tendency to augment my doubts of the strict accuracy of this analogical conclusion. As the facts which I now allude to, appear to me to be new and curious, I shall mention some of them. When the sun's rays are concentrated by a lens, they penetrate, I find, through bone, as a portion of the cranium;* through nine folds of black crape; and, what is more extraordinary, through rolled platinum. It was easy to ascertain their penetrating through the former substances, by a luminous point appearing on a surface beneath; but through the opaque platinum no light passed, yet the rays of heat passed, which was best indicated by the sensation produced, when the metal was placed on the sensitive skin, the only part of which affected was that corresponding to the focus of the lens, the metal itself not becoming sensibly warmer.† Taking, then, into consideration the difference between the painted cuticle and the dark

* This circumstance may help to explain the effect of the sun on the brain, in producing that malady commonly called *coup de soleil.*

† No effect was produced through the platinum on moist chloride of silver.

rete mucosum, and this very remarkable penetrating power of the sun's rays, it appeared to me that more direct experiments than those of Sir Everard Home were requisite, to ascertain, beyond all doubt, if the function of the colouring matter of the skin of the Negro is really such as it has been inferred to be. With a view to this, I have subjected the skin of the Negro to the direct rays of the sun, and I have made a similar trial on a mole on a fair skin. After two hours exposure to the sun, its rays moderately concentrated by a lens, (for the experiment was made in winter when the temperature was between 50° and 60°) the part acted on, in which a dark brown mole was situated, became slightly red; the following day it was red, and just perceptibly painful; and about the fourth or fifth day, desquamation of the part commenced. The desquamation took place over the mole as well as the adjoining fair part, and the mole was evidently rendered of a darker colour. On the 27th December, when the sky was clear, and Fahrenheit's thermometer in the shade at 56°, a similar experiment was made on the fore-arm of a Negro, and continued the same time. The skin acted on was a little hotter than the rest, just perceptibly darker, and it felt, he said, slightly sore. On the following day, the part appeared to be very little darker, and he said that it was slightly painful and swollen, but this last mentioned effect was not perceptible to my eye. On the 31st December, the pain had ceased; there

was not the slightest appearance of desquamation, and it was only just perceptibly darker than the adjoining skin.

From these results, I am disposed to infer, that the colouring matter of the skin of the Negro affords some protection from the scorching effects of the sun's rays, but not complete protection, and that were his skin as much disposed to inflame from the action of the sun's rays as the skin of the fairest European, this colouring matter would not prevent occasional vesication. Some of the facts already mentioned tend to support this opinion, especially the circumstance that exemption from the scorching effect of the sun is not confined to the African Negro, but is possessed equally by all the various races of men,—the inhabitants of hot climates, who are much in the open air, and exposed to bright light, whether the colour be almost black, like that of the lower classes of Singalese, or of a dull straw colour, like that of the Bojesman of Southern Africa, or of a ruddy brown, as in the instance of the Albanian shepherd of the mountains of Greece.

Nature, then, it may be remarked, is very provident, adapting the skin, impressed by the sun's rays, to bear them afterwards without inconvenience, or at least without painful suffering, the impression having apparently a protecting effect from farther annoyance, like the first attack of many of the infectious exanthemata; but with this difference, that the sus-

ceptibility to a renewal of the action is not long suspended, unless the cause is in constant activity. How long it is suspended it is difficult to determine: it is suspended in different degrees, probably in persons of different complexions and temperaments, least in the fair,* more in the brown European races, and most of all in the deeply coloured Asiatic and African tribes.

Besides the foregoing, there are other means which nature employs to counteract the influence of the sun's rays on the human body, and to keep down animal heat within the bounds of health, in situations where otherwise it would be most apt to be in excess, and mount to a feverish height. These means seem to be of two kinds, one external, the other internal.

Perspiration, both sensible and insensible, constitutes one of the first. It not only cools the surface in connexion with evaporation, but also, when in the form of sweat, by dispersing the sun's rays, prevents or diminishes their scorching agency, as is easily shewn by comparing the effect of the sun's ray's, concentrated by a lens on a dry and a wet skin. The internal means are not so obvious; but that they

* Very fair persons, as regards complexion, are least affected by climate, and exposure to the bright light of a tropical sky;—in this respect resembling the Albino. Unusual exposure to the sun's rays, is painfully felt by them; they experience sun-burn; but, after the erythema or superficial inflammation has subsided, and desquamation has taken place, the fairness of the skin is but little impaired,—and its liability but little diminished to suffer in the same manner from farther exposure.

do exist, and act beneficially, seems undoubted. There is reason to infer, that the greater the atmospheric temperature is, the less oxygen is consumed in respiration,—that the blood is less aërated, and there is less heat in consequence produced. Some facts bearing on this curious point have already been mentioned, when treating of animal temperature in summer and winter; and further on, some other facts tending to illustrate it will be brought forward, in an account of observations on the blood, in connexion with respiration. Besides this, probably, the principal balancing circumstance, others in addition may be pointed out by which nature fits man to bear with impunity, and with comparatively little inconvenience, the heat of the hottest regions of the globe of which he is a native. In a hot climate, especially within the tropics, the cuticle appears to be thinner than in a cold climate, so as to confine the animal heat less.* Moreover, I believe that the exhalants of the skin, and indeed the whole apparatus peculiar to this texture is either more developed or more active, or both, under the influence of a high than of a low or moderate degree of temperature. And the condition of the blood seems to accord, as cold appears to increase

* The cuticle of the sole of the foot and of the palm of the hand is, under certain circumstances, an exception, as when exposed unprotected to the action of a hot surface, or subjected to much pressure. I have seen a negro sleeping with his feet so close to a fire, that the outer exposed surface was almost scalding to the hand applied to it, and yet, owing to the thickness of the part, to the individual it transmitted merely an agreeable warmth.

its viscidity, heat, its fluidity ; consequently, *cæteris paribus*, in a hot climate, it will be less viscid than in a cold, more fluid and flow more freely through the cutaneous and subcutaneous vessels. Thus, by promoting perspiration, it will contribute to the cooling of the surface ; and being cooled itself, it will contribute again, when it flows back to the heart, to the reduction of the temperature of the internal parts.*

Lastly, peculiarity of constitution appears to be intimately connected with climate, and the tolerance of different degrees of temperature with impunity. The African enjoys the best health, is in the highest spirits, and is capable of the greatest exertions in hot moist regions, where the temperature is seldom below 80°; and is almost entirely exempt from those fevers of the intermittent and remittent type which have been, and probably always will be, the scourge and destruction of Europeans in hot climates.† But,

* I deduce this opinion of the blood being more liquid at a comparatively high temperature, as 88° or 98°, than at a low one, as 38° or 48°, from experiments on the blood, which will be given in the sequel, shewing that a certain degree of cold thickens the blood, and that a certain degree of heat renders it more liquid, so that in one state it is better fitted for torpid hibernating animals, and those of a cold climate, and in the other, for animals in whom the functions of life are performed with energy, and that constantly, as in a warm climate.

† In regions more unwholesome to Europeans than the Maremma, of Italy, as (to state from my own experience) in some parts of the interior of Ceylon, where not one European in a hun dred escaped fever, and the majority of those attacked, died; an in stance of intermittent, or of remittent fever was of extremely rare occurrence amongst the Black troops, who were employed in common with our

reverse the situation, and place the African negro where the European recovers his lost energy, shakes off the languor of the tropics, and is restored to health and strength, there the former commonly droops, becomes languid, feeble, and diseased, and soon sinks into the grave.* And thus it is, no doubt, that each race is propagated and multiplied in the situation most suitable to the developement of its faculties and powers.

soldiers; of which most ample proof was afforded during the rebellion of the native population of the interior of that island, between 1817 and 1819.

* In Ceylon, during the period just mentioned, whilst our soldiers were recovering their health in the cool hill forts of the mountainous parts of the island, the negroes there were dying of pulmonary consumption.

XVI.

ON A PECULIARITY OF STRUCTURE OCCASIONALLY OCCURRING IN THE BASILAR ARTERY OF MAN.

Besides those peculiarities of structure of the basilar artery which are well known, there is one of not uncommon occurrence which, to the best of my knowledge, has not hitherto been noticed by any anatomist. It is a band in the interior of the vessel, attached to its sides, and consequently intersecting it. (Vide Plate xiii. fig 2.) It varies both in its dimensions and situation. I have most frequently found it near the junction of the vertebral arteries; very seldom near the commencement of the circle of Willis. Sometimes the band perfectly intersects the vessel; at other times only partially. Sometimes I have seen it not more than a line thick; occasionally two or three lines. Its appearance, as regards its nature, has always been similar and most analogous to a fibrous structure. In every instance, I apprehend, it may be considered as congenital, and not the effect of disease. This is inferred after careful examination to endeavour to detect the effects of diseased action. In no instance were there any

indications of such action observed; the lining membrane contiguous was smooth, and there was no thickening where the ends of the band were inserted.

The basilar artery, in the manner in which it is formed, and the thinness of its coats, may be considered as approximating to a vein. The similarity is increased by the peculiarity in question. Bands of the same kind are not uncommon in the longitudinal sinus, and more delicate bands and fibres are frequently met with in the heart in the right auricle, especially about the fossa ovalis, and in connexion with the eustachian valve.

Whether this peculiarity of structure has a decided use, I am not prepared to say. In each instance in which a band or fibre presents itself, support is afforded—additional strength is imparted. The band I have described as occasionally occurring in the basilar artery must necessarily have this effect.

It was in the month of June 1837, that my attention was first attracted to the subject. Since that time, during a period of about sixteen months, I have taken every opportunity that has offered to examine the basilar artery. In 98 *post-mortem* examinations at which I have been present, made in the General Hospital, at Fort Pitt, in the time specified, I have met with it in seventeen instances. In nine, death was owing to pulmonary consumption; in two, to malignant tumour; in the remainder, in each instance, to a different disease. The ages of the individuals varied from 19 to 59; two were 25,

two 35; in point of age there was no other accordance amongst them.

I shall give the results in a tabular form, and also the proportional frequency of marked difference of size of the vertebral arteries to which my attention was at the same time directed.

TABLE I.

Period.	Number of bodies examined.	Instances of band in basilar artery.	Instances of left vertebral larger than right.	Instances of right larger than left.
1837.				
June -	5	3	4	1
July -	16	6	9	1
August -	13	2	—	1
September -	9	1	2	3
October -	5	1	1	—
November -	8	1	—	1
December -	5	—	2	—
1838.				
January -	6	1	2	—
February -	3	—	—	—
March -	1	—	—	—
April -	1	—	1	—
May - -	8	1	1	—
June -	4	—	1	—
July - -	5	—	1	—
August -	9	1	2	1
	98	17	26	8

In one instance of the total seventeen, a short filament proceeded from the intersecting band (vide Plate xiii. fig. 3), its end floating loosely.

In two cases out of ninety-eight, there was another peculiarity, which I am not aware has ever been noticed, viz.,—an opening or foramen between the two vertebral arteries, in the septum formed by their juxtaposition posterior to the basilar artery. In each instance it was sufficiently large to admit a surgeon's probe.—(vide Plate xiii. fig. 4.)

In three instances amongst the total number, each vertebral was as large as the basilar artery,—which was of the usual size of this artery; and the vertebrals did not appear to be diseased.

The proportion in which the left vertebral artery was found larger than the right, is so great, viz.,—in the ratio of 26 to 8, that it can hardly be considered accidental. But, on what the circumstance depends, I am entirely ignorant. At one time, I supposed that it might be connected with the difference of origin of the right and left subclavian; but, the notion was not supported by facts. In two instances (the only ones observed) in which the left vertebral took its origin immediately from the aorta, between the carotid and subclavian, it was smaller than the right vertebral, taking its origin from the subclavian of that side,—and that subclavian, as usual, from the arteria innominata.

In the Table, I have given the results of my observations, monthly, for a particular reason,

namely,—for the purpose of shewing how much more frequently the peculiarities of structure referred to, occurred in the bodies, which died in one month than in another. The greater frequency of rare occurrences at one time than another, is a circumstance extremely curious and mysterious, and I have often been impressed by it, as, I believe, others have been, both in hospital practice and engaged in anatomical researches. I shall mention a few instances, from memory, as examples of such impressions.

1. An oblique opening in the fossa ovalis of the heart, occasionally of such frequent occurrence as to give the idea of its being normal.

2. The eustachian valve of large dimensions in the adult, nearly as large in proportion as in the fœtal heart.

3. A fibrous filamentous connexion between the eustachian valve and the auricular septum.

4. The mouth of the coronary vein destitute of a valve.

5. Delicate tendinous filaments or threads, bordering the semilunar valves, especially of the aorta, giving the idea of an atrophied state.

6. The ligamentous cord, the remains of the ductus arteriosus, ossified.

7. Fibrinous concretions containing a purulent-like matter, formed during life, in the iliac and femoral veins, and also in the ventricles of the heart, most frequently in cases of phthisis.

8. Varicose lacteals, also in phthisis.

9. Softening of the brain, especially of the fornix, in this disease.

10. Ulceration of the larynx, also in this disease.

11. Pneumathorax from perforation of the pleura in connexion with a tubercular excavation.

12. Peritonitis, from perforation of intestine, in connexion with very limited local ulceration.

13. Ulceration and perforation of the appendicula vermiformis and consequent peritoneal inflammation.

14. Softening and wasting of the articular cartilages, especially of the patellæ.

The list might be very much extended. It may perhaps be said, that there is deception in this matter; and that the asserted greater frequency of occurrence of any peculiarity of structure, or lesion, independent of obvious causes, is more apparent than real, and that were the same attention constantly given in search, the irregularity of their occurrence would cease. This may hold good in some instances; but I cannot admit that it is applicable in those I have enumerated, and more especially in the peculiarity of structure of the band in the basilar artery, which led to the remark. In each case, the artery was carefully examined, and the result noted down at the time, and that specially. And, moreover, in relation to the subjects of observation, what could be less favourable for uniformity of result? The individuals were not of

any particular family or race; they were men of different regiments, English, Irish, Scotch, brought to the general hospital, the invaliding station of the army, from different parts of the globe, in a manner approaching to the accidental as nearly as possible.

I am disposed to believe, that were the pathological anatomist engaged in extensive and minute research, to institute a series of observations on organic changes, analogous to that which Sydenham conducted on ordinary maladies, he might arrive at the conclusion, that there are organic constitutions prevalent at times, not less than atmospheric, and which (however produced) may be as much concerned in the origin of chronic disease, as the atmospheric influences are in the acute.

I shall give in a tabular form some of the results of my experience bearing on this subject, drawn from my notes of the various post-mortem examinations which I have attended, during a period of nearly eighteen years, namely, from May, 1821, when I commenced the practice of making a note of every fatal case, in which there was an examination of the body after death. My experience has been chiefly confined to our military hospitals, indeed at home it has been entirely so restricted; on foreign stations, it extended to the native population, especially in Malta, where the civil hospital offers an ample field for research.

TABLE II.

Stations.	Years.	Number of bodies examined.	Instances of tubercles in the lungs.	— of ulceration of larynx with do.	— of ulceration of larynx without do.	— of pneumathorax.	— of varicose lacteals.	— of aperture in fossa ovalis.
England, Fort Pitt	1821	29	11	—	—	1	—	1
—	1822	61	33	19	—	—	6	10
—	1823	40	20	7	—	1	—	2
—	1824	10	5	2	1	—	—	—
Ionian Islands.	1824	18	2	—	1	—	—	2
—	1825	36	5	2	—	—	—	1
—	1826	9	3	—	—	—	—	—
—	1827	33	8	1	—	—	—	3
—	1828	6	—	—	—	—	—	—
Malta.	1828	32	5	1	—	—	1	5
—	1829	19	4	1	—	—	—	3
—	1830	41	5	1	1	—	—	9
—	1831	54	19	3	—	1	2	8
—	1832	55	8	1	—	3	—	7
—	1833	49	7	9	—	1	—	5
—	1834	57	15	5	—	3	1	5
—	1835	10	4	1	—	—	—	—
England, Fort Pitt.	1835	31	20	2	—	2	—	1
—	1836	72	49	18	—	1	—	8
—	1837	81	50	15	1	5	—	8
—	1838	43	45	11	—	1	—	3
Total	18	786	318	99	4	19	10	82

On the results contained in this Table, I shall restrict myself to a few remarks.

The very large proportional number of cases in

which tubercles were found in the lungs, viz. 40 per cent. of the whole, may excite surprise. I must confess it had that effect on my mind, and the more so, as no doubtful instances were admitted: I rigorously rejected every example not coming under the denomination of the consumptive tubercle—that is, a tubercle, albuminous in composition, admitting of induration by boiling, as pointed out by Dr. Abercrombie, and of softening in the progress of disease, giving rise to vomicæ and excavations in the lungs. The melanotic tubercle was excluded, and also certain concretions more or less resembling tubercles, whether consisting principally of phosphate of lime and the other materials of bone, or of a nature approaching to cartilage. I may also remark, that no cases were admitted as supposed instances of tubercles, from the presence merely of cavities in the lungs. Cavities existing unaccompanied by tubercles were inferred to be examples of pulmonary abscess, of which several instances occurred.

No doubt, the astounding frequency of tubercles recorded in the Table, is partly owing to the description of cases sent to the General Hospital, at Fort Pitt, where a considerable proportion of the whole mortality under observation occurred. But, making the most ample allowance, on this account, I apprehend the conclusion is unavoidable, that the existence of tubercle is far more frequent than is commonly supposed, and the reported deaths from phthisis would indicate.

Of the four instances of ulceration of the larynx unaccompanied by tubercles, the first, was complicated with empyema and purulent effusion into the pericardium to the extent of three pounds and a half; the second was associated with melanotic tubercles in the lungs; and the third and fourth were connected with small-pox.

All the instances of pneumathorax, with the exception of one, occurred in cases of tubercular phthisis, and originated in a communication of a valvular kind being established between the pleura, and a bronchial tube by ulceration, commonly through the medium of a cavity. In the one exception, a similar communication was detected, the consequence of a partial destruction of lung, from an abscess in the liver penetrating and bursting into the lung through the diaphragm.

Of the large number of examples of aperture in the fossa ovalis, all were oblique, with the exception of three. Of the three direct, one was sufficiently large to admit the fore-finger, and two to admit the end of the little finger. In neither instance was there the slightest appearance of the morbus cæruleus. The subject of the first-mentioned and most remarkable example was an old soldier, who for many years enjoyed excellent health. In these cases, it appears inevitable that there must have been an admixture of venous and arterial blood in the auricles. In the examples of the oblique passage, the probability is, that no blood flowed from one auricle

into the other. In one instance, in which the oblique aperture was sufficiently large to receive the end of the little finger, the right cavities of the heart were found distended with coagulated blood, and the left empty.

XVII.

OBSERVATIONS ON THE FLUID IN THE VESICULÆ SEMINALES OF MAN.

I believe I am justified in stating, that there is still a difference of opinion amongst physiologists respecting the nature of the fluid of the vesiculæ seminales, especially in this country, where the authority of John Hunter is necessarily high and influential.

The difference of opinion alluded to is, whether the fluid in question is secreted by the testes, or by the vesiculæ; and whether, in consequence, the vesiculæ are to be viewed chiefly as reservoirs or merely as glands.*

* Vide Dr. Bostock's Elements of Physiology, Vol. iii. p. 7, and foot-note, where the question is left unsettled by the learned author. He thus expresses himself on the subject:—"Besides the testes, the vesiculæ seminales, both from their size and their situation, have been supposed to perform some important part in the function of generation, although it has been difficult to ascertain the exact nature of the purpose which they serve. The opinion formerly entertained was, that they are merely reservoirs, in which the semen is deposited as it is secreted. In consequence, however, of the observations of Hunter, who remarked that the fluid contained in these cavities appeared to be different from that found in the testes, many of the

Conceiving that some light might be thrown on the subject by the examination of the fluid in the vesiculæ and in the vasa deferentia, after death, in a variety of cases, I have availed myself, for the purpose, of such opportunities as have offered in the General Military Hospital, at Fort Pitt, under my superintendence. And I shall now relate the results,—with the belief, that they may aid in settling the disputed question;—and, also with the hope, that they may be found to be not entirely devoid of interest in connexion with an obscure branch of pathological inquiry.

It is necessary for this double purpose to prefix in each instance a slight notice of the fatal case. I shall make no selection of cases; but give them nearly in the order as to time in which they occurred. And, I may remark, that, as every fatal case was subjected to a post-mortem examination, according to the usage of the hospital, these brought forward (the total deaths from the 22d September, 1838, to the 6th of the ensuing December) afford a tolerably correct example of the general mortality of this hospital,—the patients in which are chiefly invalids labouring under chronic diseases, incapacitating them for farther military service.

1.—Aged 30; previously labouring under pulmonary disease and an impaired constitution, was

later anatomists have supposed that the vesiculæ seminales produced a secretion of a peculiar nature, the use of which may probably be to dilute the semen, or to add to its bulk."

admitted with pneumonia, and died on the third day. On dissection, thirty-nine hours after death, the inferior lobe of the right lung was found hepatized, weighing nearly three pounds, the superior and middle only half a pound. There was a tubercular excavation and numerous tubercles in the left lung, — and small cavities and a pretty distinct cicatrix of a vomica in the upper part of the superior lobe of the right lung. The body was not emaciated.

The vasa deferentia and vesiculæ seminales were removed six hours after death. About a drop of fluid was obtained from each vas deferens, which accorded in appearance with the received description of the spermatic fluid. Examined with a microscope, constructed by that excellent maker Mr. Ross, and using an object-glass of one-eighth inch focal distance, it was found to contain numerous animalcules, some of them in active motion. The vesiculæ contained a considerable quantity of fluid. Some of it was collected after six hours; another portion later, after forty-two hours. The former did not appear to me to differ from the fluid of the vasa deferentia. It abounded in spermatic animalcules, some of which were alive and active: on standing a few hours it separated into two parts, one opaque that had subsided, the other transparent; and this was copiously precipitated by alcohol, and rendered of a consistence almost gelatinous. The last collected had

a brownish tinge; it, too, abounded in animalcules; but they were motionless and dead; warmth had no effect in reanimating them.

2.—Aged 57; a violent maniac, who died thirteen days after admission; the disease was of about a fortnight's duration,—a third attack. The dissection was made fifty-seven hours after death. The body was much emaciated. The lateral ventricles of the brain were greatly distended with fluid; they communicated freely,—the septum lucidum having been destroyed by disease. No other well-marked lesion was discovered.

A very minute portion of fluid was obtained from the vasa deferentia. It was of the colour, and very much of the appearance and consistence of pus. It contained very many spermatic animalcules — all dead. The fluid from the vesiculæ was small in quantity, browner than that from the duct, as if slightly tinged by the colouring matter of the blood. It contained vestiges in abundance of spermatic animalcules, and a few of distinct form.

3.—Aged 39; died of pulmonary consumption, complicated with pneumathorax of the right side and empyema.* The dissection was made thirty-six hours after death. The body was much emaciated. Besides the disease in the lungs, the ileum

* In this case, as in every other of pneumathorax which I have yet examined, the air in the pleura was derived from the atmosphere through a valvular ulcerated opening in the lung.

in its inferior part was severely ulcerated, and the cæcum slightly. There had been diarrhœa in the last stage of the disease.

The vasa deferentia, and the vesiculæ seminales, were removed and examined six hours after death. The contents of the latter were small in quantity, and of nearly gelatinous consistence. The fluid of the ducts was more liquid, and nearly of its usual cream or purulent-like appearance. In neither of them could any animalcules be discovered, or even vestiges of them, or any distinct globules.

4.—Aged 20; died of pulmonary consumption. Extreme debility and emaciation marked the last stage of the disease, in which diarrhœa and night-sweats alternated. The dissection was made twelve hours after death. No notable lesion was detected independent of the pulmonary disease, which was vast and various,—a complication of tubercles and cavities, of œdema and hepatization. The intestines were not ulcerated.

The vesiculæ and vasa deferentia were examined eleven hours after death. The former contained a small quantity of fluid, which was brownish, thick, and slightly viscid,—the latter an extremely minute portion, like thin starch. In both, under the microscope, there appeared, as it were, the fragments of animalcules and numerous globules.

5.—Aged 32; died of latent pulmonary consumption, after having been a patient in the lunatic asylum of the establishment about a month. The

dissection was made fourteen hours after death. The body was much emaciated. Besides tubercles in different stages, and excavations in the lungs, and partial hepatization, there was little other organic disease. Diarrhœa had not preceded death; there were, however, some small ulcers in the colon, and marks of old ulcers healed.

The vesiculæ and the vasa deferentia were examined sixteen hours after death. A small drop of fluid was obtained from the latter, of cream or purulent-like appearance, which abounded in seminal animalcules. The vesiculæ were moderately turgid; the fluid in them was also opaque, white, like purulent matter, and abounded in animalcules. The animalcules in each instance were dead. Neither fluid changed the colour of litmus or of turmeric paper.

6.—Aged 39; died of gangrene of the superior lobe of right lung, complicated with effusion into the pleura, and with ascites. There were sixty-one ounces of turbid serum in the right pleura, and seven pints of serum in the cavity of the abdomen. The liver, spleen, and pancreas were denser than natural; the weight of the liver was only two and a-half pounds. There was a large ulcer in the cæcum, and numerous marks of old ulcerations healed in the colon and rectum. Chronic dysentery preceded the fatal disease. The dissection was made twenty-eight hours after death.

The right vas deferens was examined two hours

after death, and the vesiculæ half an hour later. A small drop of fluid was obtained from the seminal duct. It resembled in appearance the purest purulent matter: it had no distinct smell; it rendered turmeric paper slightly brown. Under the microscope it appeared to consist chiefly of minute globules, very much smaller than those of purulent matter, and to be entirely destitute of animalcules. The vesiculæ were so shrunk and collapsed, that difficulty was experienced in collecting sufficient fluid even for microscopic examination. No animalcules could be detected in it, and no distinct globules. It was colourless and not viscid,—more like serum than mucus; and it was alkaline, and changed distinctly the colour of turmeric paper.

7.—Aged 42; of twenty years military service; died of pulmonary consumption. The dissection was made thirty-seven hours after death. The body was much emaciated. The lungs weighed five pounds. Besides tubercular excavations and a vast quantity of tubercles in different stages of progress, there was partial œdema of the lungs and hepatization. There were large ulcers in the jejunum, ileum, and colon; diarrhœa did not precede death. The prostate gland contained a sinus, which opened into the neck of the bladder. The testes were adhering to the tunicæ vaginales; and the substance of the right testis was partially indurated, as if from the effect of common inflammation.

The vesiculæ seminales were moderately dis-

tended with fluid, of a light brownish hue, slightly turbid and opaque. It abounded in well-formed spermatic animalcules, and contained a few blood corpuscles, or particles extremely like them. It had no effect either on turmeric or litmus paper. An extremely small portion of fluid was obtained from one of the vasa deferentia, which, in appearance, was very like that of the vesiculæ. Under the microscope, a few animalcules were seen in it, many blood corpuscles and some smaller particles.

8.—Aged 32; died of bronchitis, complicated with hepatization of the right lung, and an aneurism of the arch of the aorta pressing on the trachea. The dissection was made thirty-two hours after death. The body was not emaciated. The bronchia were nearly filled with muco-purulent matter. The right lung weighed three pounds; the greater part of it was hepatized in different degrees, and it contained a few clusters of granular tubercles. No well-marked lesion could be discovered in the abdominal viscera.

The vesiculæ were moderately distended with fluid of purulent-like appearance, which abounded in seminal animalcules. The fluid obtained from one of the vasa deferentia, extremely minute in quantity, was found to contain a few animalcules, and very many particles smaller than those of the blood.

9.—Aged 33; died of pulmonary consumption. The dissection was made fifteen hours after death. The body was sub-emaciated. Besides numerous tubercles in different stages of progress and several

large cavities in the lungs, there was partial œdema and hepatization of them. The liver weighed six and a half pounds, and contained some fatty matter. There were no dropsical symptoms. There were numerous ulcers in the ileum and colon. There had been no diarrhœa.

A small quantity of fluid was found in the vesiculæ, of a brownish hue and semi-opaque. It did not change the colour of turmeric or of litmus paper. It abounded in spermatic animalcules. The very little fluid procured from a vas deferens was like dilute purulent matter in appearance; it contained a few animalcules, and very many minute globular particles.

10.—Aged 20; died of pulmonary consumption. The dissection was made twenty-six hours after death. The body was much emaciated. Both lungs abounded in tubercles, had a large excavation in each superior lobe, and contained many small vomicæ, and the right lung in addition was extensively hepatized. There was extensive superficial ulceration of the larynx and trachea. The ileum and colon were deeply as well as extensively ulcerated. The appendicula vermiformis was perforated by ulceration, and through the medium of a sinus communicated with the colon by an ulcerated opening. There were thirteen ounces of a mixture of purulent matter and serum in the cavity of the pelvis. Severe diarrhœa and tormina preceded death.

The vesiculæ and vasa deferentia were examined

four hours after death. The former afforded only a small quantity of fluid; what was first pressed out, was like thin starch in appearance; what was last expressed was gelatinous. The former, under the microscope, was found to contain many animalcules, mixed with globular particles and small masses of mucus. The latter contained very few animalcules, and consisted chiefly of mucus. The fluid from the vas deferens was very like that which flowed first from the vesiculæ, excepting that it was without any tint of brown which that fluid possessed. Under the microscope, only one animalcule, perfect in form, could be detected; fragments of others, as it were, appeared, and some globules, and a good deal of transparent mucus in minute masses.* The fluid of the vesiculæ and of the vas deferens was distinctly alkaline; each changed the colour of turmeric paper.

11.—Aged 27; died of pulmonary consumption. The dissection was made ten hours after death. The body was much emaciated. The lungs were enormously diseased; besides containing large cavities and many vomicæ, and a vast quantity of tubercles in different stages of progress, they were partially œdematous and hepatized. There were a few ulcers in the ileum, and a thickened granular and partially ulcerated state of the rectum. There had been diarrhœa in the last stage of the disease.

* In this and every other instance the vas deferens was removed for examination, before entering the cavity of the abdomen; the cord was laid bare, the duct separated, and a ligature applied previous to dividing it.

Very little fluid was found in the vesiculæ seminales. It was partly thin, of a light brownish hue; and partly thick and gelatinous,—the latter from the fundus, the former from the anterior portion. Under the microscope, a few spermatic animalcules were detected in the thin fluid, mixed with what appeared to be fragments of them. No animalcules were observed in the gelatinous fluid; it seemed to consist principally of mucus. Neither fluid had any effect on turmeric paper. From the vas deferens hardly sufficient fluid could be procured, even for microscopic examination. It contained a few minute globular particles without any animalcules.

12.—Aged 33; died of diffuse cellular inflammation of the neck, with suppuration of the cervical glands. The dissection was made twenty-six hours after death. A superficial cavity was found in the cerebrum, under the posterior part of the right parietal bone, which contained about a drachm and half of fluid, which became turbid on the addition of nitric acid. There had been no suspicion of cerebral disease during life. The cellular tissue of the neck generally was saturated with thick purulent matter; and most of the cervical glands were diseased and contained abscesses. The right sterno-cleido-mastoideus muscle, on its under side, appeared wasted and corroded, and presented a suppurating surface. A small mass of fibrin, softened internally, adhered to the inner coat of the right internal jugular vein. Neither the integuments, larynx, œsophagus, or

pleuræ, participated in the disease. During life it had not been suspected. The left femoral and the right iliac and femoral veins, were obstructed with coagula, which in the inferior parts consisted of crassamentum, and in the superior of fibrin. In the upper part of each vein the fibrinous concretion contained a cavity in which was a semifluid, having a good deal the character of purulent matter, and which would commonly be called purulent matter.* The lower extremities were œdematous, the right more than the left,—but even the right extremity only in a moderate degree. There was little hair on the pubis, or on the chin; the partes naturales were all small; the larynx was small; the skin delicate. According to his comrades, he had always shown an aversion to the sex.

A very minute portion of fluid only could be procured from the vasa deferentia, which, under the microscope, exhibited numerous small particles, and a few larger globules,—but no spermatic animalcules. The fluid of the vesiculæ was also small in quantity and destitute of animalcules. It was of a light-brownish

* It sustained this character subjected to the best tests: Thus, agitated with water, a white matter subsided, very little prone to putrefaction, iridescent when held before a light, globular under the microscope. The differences were rather in degree than kind, indicating, as it were, if the expression may be used, an imperfect purulent matter; thus, it putrified more readily than purulent matter; was only just perceptibly iridescent, and its particles were more irregularly globular and less compact. The mode of formation of this kind of fluid, with which pathologists are now tolerably familiar, is a curious problem, and deeply interesting in many of its relations.

hue, slightly opaque, contained some globules, and did not change the colour of turmeric or of litmus paper. The fluid from their fundus was most gelatinous, and appeared to consist chiefly of mucus.*

13.—Aged 29; died of pulmonary consumption, complicated with peritonæal inflammation. The dissection was made twenty-seven hours after death. The body was exceedingly emaciated; the lungs were very voluminous and heavy; they contained a vast number of tubercles in different stages, some small cavities and one large one. This cavity was lined with a false membrane, which was even spread over the abrupt mouths of two or three bronchial tubes, and it was empty. Whether it was distended with air, or its sides pressed together, was not ascertained. Eight pints of serum were collected in the cavity of the abdomen. There were very numerous adhesions between the viscera. The omentum abounded in granular tubercles, and resembled an elongated pancreas. The cæcum was misplaced, and with the colon was confined to the left side. The ileum, appendicula vermiformis, and colon, were severely ulcerated. The appearances indicated a perforation of the appendicula by ulceration, afterwards closed by adhesion, and which probably gave rise to the peritonæal inflammation. The abdominal disease was not suspected during life.

No fluid could be obtained from the vasa defer-

* The vesiculæ seminales, and their contents, in this instance, resembled those of such castrated animals as I have hitherto examined.

entia; one was examined three hours after death. The testes were of natural size, and not apparently diseased. The vesiculæ contained very little fluid. It was thick, gelatinous, nearly transparent and colourless, and did not change the colour of turmeric or litmus paper. Under the microscope some globules were seen in it, but no animalcules.

14.—Aged 27; died of pulmonary consumption. The dissection was made thirty-two hours after death. The body was much emaciated. The lungs were enormously diseased; besides a large excavation in each superior lobe, and several smaller ones, and a vast quantity of tubercles in progressive change, there was partial œdema and hepatization, and thickening of the bronchia. One arytenoid cartilage was laid bare by ulceration; the left nervus vagus was partially atrophied under the pressure of an enlarged bronchial gland. There were numerous large granulating ulcers in the cæcum and ascending colon; the vermiform appendix was severely ulcerated, and the lower part of the ileum in a less degree. A short time before death, the bowels were regular; previously there had been diarrhœa.

The fluid obtained from the vasa deferentia, less than a drop in quantity, had a healthy appearance: it contained a few spermatic animalcules, and many globules, most of them very small, a few large. The vesiculæ were moderately distended with a fluid of a just perceptible brownish hue, of the consistence of mucilage, and slightly viscid. It did not change the

colour of turmeric paper. A small number of spermatic animalcules were detected in it, and many large globules, which did not disappear on dilution with water.

15.—Aged 27; died of the effects of inflammation of the pleuræ and lungs. The dissection was made thirty-six hours after death. The body was sub-emaciated. There were twelve ounces of serum in the left pleura, and sixteen ounces in the right, with a sediment in each of flakes of lymph. Both lungs were partially adhering, and much heavier than natural; the left was œdematous to a considerable extent, and the right hepatized. Neither contained tubercles. In the superior lobe of the right lung, there was a minute cavity, surrounded by an indurated, dark, puckered structure, conveying the idea of a cicatrix of an abscess. The cæcum and ascending colon were red and rather flabby. The spleen, pancreas, and kidneys were obscurely diseased. During life there had been no suspicions of thoracic disease. Symptoms of disease of various kinds had existed at least nine months before death; anasarca prevailed at one time, and ascites; latterly the bowels were irregular, occasionally relaxed and passing blood, occasionally constipated.

The vasa deferentia were examined twelve hours after death. The very minute portion of fluid obtained from them was destitute of distinctly formed animalcules; it contained globules of different sizes, some of them very like fragments of the peculiar

entozoa,—the circular portion without the filament. The vesiculæ were examined thirty-six hours after death. They contained pretty much fluid, of a just perceptible brownish hue, and of a gelatinous consistence. Very many well-formed animalcules appeared in it. It did not change the colour of turmeric paper.

16.—Aged 33; died of complicated organic disease of the bones, lungs, pleura, heart, peritoneum, &c. The dissection was made thirty-six hours after death. The body was exceedingly emaciated. The os frontis was carious, and in two places penetrated through by ulceration; lymph was deposited underneath on the dura mater. The left clavicle was enlarged, softened, spongy, and unusually vascular. At the base of each semilunar valve of the aorta was a small bony concretion. The heart appeared atrophied. The pleuræ were studded with small, white, firm tubercles. The right pleura contained sixteen ounces of serum; the left four ounces. In the inferior lobe of right lung, there were two circumscribed masses of coagulated blood. The cavity of the abdomen contained twelve pints of serum. The abdominal peritoneal lining generally was studded with tubercles similar in character to those of the pleuræ, as was also the omentum, which, gathered into a long mass retained by adhesion, resembled a pancreas in its appearance. The kidneys were small and obscurely diseased. The left iliac and femoral vein, and the right femoral, were com-

pletely obstructed by coagula, in part consisting of fibrin, softened, and of imperfect purulent-like appearance, and in part of crassamentum little altered. There were no tubercles in the lungs; no ulcers in the air-passage or alimentary canal.

The vasa deferentia were examined six hours after death, and the vesiculæ thirty-six hours after. The very little fluid procured from the former, contained numerous minute globules, and some of a larger size, but no animalcules. No animalcules either were found in the fluid of the vesiculæ, merely globules of different sizes. The fluid contained in them was in a very small quantity, of a light brownish hue, and it did not change the colour of turmeric paper.

17.—Aged 31; died of pulmonary consumption. The dissection was made twenty-five hours after death. The body was greatly emaciated. Besides tubercles, which abounded in the lungs, and tubercular excavations, there was some œdema of these organs, and hepatization and emphysema,—the last to a considerable extent,* with much redness of, and purulent discharge on the bronchia. The intestines were not ulcerated.

* Contiguous to the part where the emphysema was most remarkable, was a small cavity, which communicated by an ulcerated opening with the air-cells of the lungs. When the part was punctured under water, several cubic inches of air escaped from the emphysematous portion. It consisted of eight of azote by measure, and two of carbonic acid gas. In many other instances in which I have examined the air of emphysema of the lungs, I have found it similar.

The vasa deferentia and vesiculæ were examined twenty-seven hours after death. The vesiculæ were considerably distended with fluid, which was of a brownish tint, and did not change the colour of turmeric paper. It differed in consistence, according to the part from whence taken; that from the fundus was of the consistence of thick mucilage; that from the anterior part was more fluid; each abounded in well-formed animalcules, and in globules of a large size. About a quarter of a drop only of fluid could be procured from each vas deferens. No animalcules could be discovered in it, but globules of different sizes, and some resembling fragments of animalcules.

18.—Aged 49; died after an illness of three days, in consequence of inflammation of the brain; had been previously in good health. The dissection was made fourteen hours after death. The body was stout and muscular. A large quantity of turbid serum with a sediment of purulent mater was found in the ventricles, and a considerable deposit of lymph with which purulent matter was mixed, on the base of the brain, the medulla oblongata, and on the upper part of the spinal cord. The abdominal and thoracic viscera generally were sound.

The testes and vasa deferentia were examined ten hours after death; the vesiculæ seventeen hours after. The fluid obtained from the vasa deferentia abounded in animalcules, some of which were alive and in languid motion. The fluid of the vesiculæ

also abounded in them; they were all dead. This fluid had a brownish tint, and reddened litmus.*

19.—Aged 30; died of pulmonary consumption. The dissection was made twenty hours after death. The body was much emaciated. In both lungs granular tubercles abounded, and there was a large cavity in the superior lobe of each. The liver, spleen, kidneys, and supra-renal glands were unusually firm. The liver weighed seven pounds, and contained some fatty matter. The spleen was about twice its natural size, and a portion of it dried on paper left a faint grease-stain. The ileum, cæcum, appendicula vermiformis, colon, and rectum were partially ulcerated; and some of the ulcers in the large intestines had penetrated through all the coats, excepting the peritonæal, and, indeed, this coat was penetrated through in the appendicula, where it happened to adhere in the right iliac fossa. There had been no dropsical symptoms; no peritonæal inflammation; diarrhœa had been troublesome in the last stage of the disease.

The vasa deferentia and vesiculæ were examined twenty-two hours after death. One vesicula contained pretty much fluid, the other very little. The fluid was of a just perceptible brownish hue, and of

* In this case, the prostate contained a good deal of fluid, corresponding to Meckel's description of it in its healthy state. It was of a fawn colour, opaque, slightly viscid; under the microscope it was found to abound in pear-shaped particles, nearly equal in magnitude to blood-corpuscles.

the consistence of mucilage; it contained a large number of animalcules and some globules, and did not change the colour of turmeric or litmus-paper. An extremely minute quantity of fluid was procured from the vasa deferentia, of cream-like appearance; it contained some globules, and many minute particles, but no animalcules.

20.—Aged 41; died in consequence of the rupture of an aneurism of the thoracic aorta, not suspected during life. The dissection was made thirty-three hours after death. The body was muscular and robust as in perfect health. In the left pleura, into which the aneurism had opened, there were three and a-half pints of serum, and five and a-half pints of soft crassamentum. The viscera generally were sound.

The vesiculæ were examined thirty-eight hours after death, and the right vas deferens fifty-eight hours after. The fluid in the former was pretty abundant, of a grayish hue, without tint of brown, and of the consistence of common mucilage. It contained many animalcules; did not change the colour of turmeric or of litmus-paper; was coagulated by alcohol, and rendered a little more opaque by momentary boiling. In the drop of opaque fluid obtained from the vas deferens, no animalcules could be detected; it contained very many minute particles, and many particles of an irregular form.

In all these cases the testes were examined, as well as the vesiculæ and the vasa deferentia. Except-

ing in two instances, viz. the 18th and 20th, no animalcules could be seen in the fluid expressed from the divided substance of the gland. The fluid, when it could be obtained in sufficient quantity for accurate observation, was transparent, generally contained globules nearly equal in diameter to the blood corpuscles, and invariably contained dense particles, apparently spherical, very much smaller, from twelve to fifteen times smaller, and which, it may be conjectured, were the ova of the spermatic entozoa.* In the two instances in which spermatic animalcules were found in the fluid of the tubuli, the quantity of the fluid was greater than in the others.†

What are the inferences to be drawn from the preceding observations in relation to the question

* I am disposed to consider this as probable, partly in consequence of the peculiarity of appearance of the particles differing from any particles which I have hitherto seen in other parts of the body,—and partly from analogy. MM. Prevost and Dumas, from their very extended and ingenious researches, have adopted a different conclusion, viz. that the spermatic entozoa are the result of secretion, and that the testes are the secreting organs;—a doctrine, perhaps, equally difficult to prove or disprove. The circumstance of the animalcules having been detected in the majority of the preceding cases in the vasa deferentia and vesiculæ, when they could not be found in the testes, appears to me more in favour of their origin *ex ova* than by secretion.

† Probably in health, these animalcules exist invariably in the testes of man contained in the tubuli. The results of the experiments of MM. Prevost and Dumas on twenty-five different species of vertebrate animals, may be adduced in support of this conclusion; (Vide Ann. des Sciences Naturelles, vols. i. and ii.) and I may add, that all the comparative trials I have yet made are in favour of it.

started concerning the nature of the fluid, and the use of the vesiculæ seminales?

The first inference that appears to me unavoidable is, that the vesiculæ are seminal reservoirs, according to the old opinion on the subject and that which is still most commonly entertained by the continental physiologists. And next, that they are not merely reservoirs, but are also secreting organs, furnishing mucus, and perhaps some other fluid, for admixture with the semen.

The first inference is supported by the general resemblance, in several cases, of the fluid of the vasa deferentia and of the vesiculæ, and of the existence of the characteristic spermatic animalcules in the fluid of the vesiculæ, in every instance in which they were detected in the fluid of the vasa deferentia.* Hunter does not mention having used the microscope in his inquiry. If he had, he could hardly have failed to have arrived at a different conclusion.

The second inference is supported by there being a certain difference in almost every case between the fluid of the vesiculæ and that of the vasa deferentia, and especially by the circumstance, that the difference of quality is most perceptible in the fluid of the

* I may add, that I have observed spermatic animalcules in the vesiculæ of the ram and bull, precisely similar to those found in their testes and vasa deferentia; and if I recollect rightly, they have been detected in the vesiculæ of some other animals by MM. Prevost and Dumas. Whether the vesiculæ of certain animals, however, have not a specific use, distinct from that of being merely reservoirs, appears to be deserving of further and special inquiry.

fundus,—where most out of the way of being readily mixed with the fluid of the testes. What the exact difference of qualities is between the fluid of the vesiculæ and of the vasa deferentia, and, it may be added, of the vasa beferentia and of the testes, in perfect health, remains to be ascertained. It can be determined only by careful examination and comparison in the instances of criminals who have been executed, or of persons who have been killed by accident, not labouring under chronic disease, and in the vigour of life. I am disposed to think that the difference will not be found very considerable, and that between the fluid of the vesiculæ and of the vasa deferentia, it will consist chiefly in the former being more dilute and perhaps more bland and mucous.* Hunter was of opinion that the fluid of the vesiculæ is naturally of a brownish hue. As an invariable quality this appears questionable. The sooner after death the fluid is examined, the less brown it is. I have given amongst the preceding cases, instances in which it was colourless, and those examples in which there was reason to consider it least altered in consequence of disease: and Mr. Hunter's best observations are in accordance, viz. two instances, the only ones given in which examination was made very soon after death. He says, in a man killed by a cannon-ball, "the fluid in the

* That the vesiculæ are glandular, or possess mucous follicles, seems unquestionable; I have seen them with the microscope, and the mucous secretion is made manifest in disease.

vesiculæ was of a lighter colour than is usually found in men who have been dead a considerable time; but it was by no means like the semen either in colour or smell." And, he adds, "in another man who died instantaneously in consequence of falling a considerable height, and whose body I inspected soon after the accident, I found the contents of the vesiculæ of a lightish whey-colour, having nothing of the smell of semen, and in so fluid a state as to run out on cutting into them."* The colour which the fluid of the vesiculæ commonly exhibits in late post-mortem examinations is probably partly derived from the infiltration of some of the colouring matter of the blood after death, and, in part, perhaps occasionally, from a vitiated secretion of the follicles, or secreting surface of the vesiculæ, and of the adjoining vasa deferentia.†

It would be a work of supererogation to enter into a detailed examination of the arguments brought forward by Hunter in support of the views he advocated relative to the vesiculæ and their contents: it has been ably done by Meckel in his Manual of Anatomy, by whom each argument has been seriatim

* Observations on certain parts of the animal economy. By John Hunter, 4to. London, 1786, p. 28.

† Occasionally after death, not only is the fluid of the vesiculæ found of a brownish discoloration, but also the inner surface of the vesiculæ, and of that portion of the vasa deferentia which is contiguous, and of similar structure; indeed, I have seen the portion of the vasa deferentia just mentioned more discoloured than the vesiculæ themselves.

answered, and in most instances, as it appears to me, in a satisfactory manner, but without reference to microscopical research.*

Admitting the fact, that the vesiculæ are, like the gall-bladder and bladder of urine, recipients, it may be viewed as a fortunate circumstance in our economy, and admirably adapted to the condition of *man*. Like the bile or the urine, so the spermatic fluid in the healthy adult appears to be in constant process of secretion, and to pass as it is formed into its appropriate reservoir, from whence, without disturbance of the system, in a state of continence, it is either pressed out and voided during the act of alvine evacuation, or it may be in part absorbed. Mr. Hunter, in accordance with the opinion which he had formed of the use of the vesiculæ, did not admit this. He believed that the fluid rather accumulated in the testes, and gave rise there to annoyance, requiring its evacuation by a disturbing act, a doctrine of some danger, especially to youth, in its consequences. In confirmation of what is stated above, and in opposition to the doctrine of Hunter, I may farther state, that I have frequently examined microscopically the fluid from the urethra following the alvine evacuation, and I have always found it, from a healthy person, abounding in animalcules,

* Manuel d'Anatomie Générale, Descriptive et Pathologique. Par J. F. Meckel, traduit par A. Jourdan et G. Breschet. Paris, 1825. Tom. iii. p. 643, et seq.

the majority of which have always been dead;* and thus, perhaps, seeming to indicate that the vesiculæ are cloacæ as well as reservoirs, and are essentially designed for man, to enable him to control and to exercise that moral check on the passions, by which he should be distinguished from brute animals, and without which no considerable advance can be made in civilization, or in elevation of individual condition and character.

Relative to the effects of disease on the fluid of the vesiculæ seminales, and on the spermatic fluid generally, the instances brought forward are too few to admit of extensive induction. They seem to shew, 1st, that tubercular consumption, even in its advanced stage, has little effect, in preventing the secretion of the testes, or the production of those animalcules, on which, there is much reason to infer, the active power of the semen depends; 2dly, that other wasting diseases, terminating in death, have the effect of arresting the secretion; 3dly, that the contents of the vesiculæ, and vasa deferentia, under the influence of disease, retain longer their characteristic qualities than the contents of the tubuli; and 4thly, that there is least fluid in the vesiculæ and in the vasa deferentia, and that it is most altered in

* On rest, contrary to what Hunter states, I have found this fluid separate into two parts—a sediment containing the animalcules and opaque, and a clear supernatant fluid destitute of animalcules—thus analogous also to the spermatic fluid.

instances of chronic diseases of the abdominal viscera, and especially of the intestines.

The subject of inquiry is in many respects curious, and some other of its relations are not uninteresting or unimportant. I shall allude to one only before concluding, and that connected with medical jurisprudence.

Admitting that spermatic animalcules are characteristic of and essential to healthy spermatic fluid, in certain doubtful criminal cases, probably, decisive evidence may be obtained by means of microscopical examination. The spermatic fluid undergoes change rapidly when exposed to the air, and even soon becomes putrid; but the spermatic animalcules, I find, resist change in a remarkable manner. In one instance, distinct remains of these animalcules were observed in putrid fluid, which had been kept ten weeks, at a temperature varying between 50° and 60° of Fahrenheit, and in another which had been kept a year and half. In another instance, some fluid of the vesiculæ was applied to linen, and wrapped in paper and put by in a close drawer. It was examined the following day; at the end of a week, and after eighteen days, and each time animalcules were discovered under the microscope. The mode of making the trial was by saturating a small portion of the smeared linen with a few drops of water, and gently pressing out a drop for the experiment. Fragments of the animalcules were very distinct, and sufficiently characteristic; and on careful inspec-

tion, an entire animalcule, here and there, was observed.* The application of these facts to the purposes of evidence does not require any comment.

In conclusion, it may not be amiss to allude to the views of Sir Everard Home on the subject of the fluid under consideration. They occur detailed at great length and in a very circumstantial manner in the fifth volume of his Lectures on Comparative Anatomy, and are well worthy of being referred to as a scientific curiosity. The opinion which Sir Everard Home there maintains is, that spermatic animalcules have no real existence; and that all inquirers who have asserted their existence, from the time of their discovery by Ham, in about 1677,† to the present period, have been in egregious error, having mistaken, as he believes, filaments of mucus for entozoa, and fallen thereby into wild theories. Sir Everard Home relates particularly the grounds on which he came to this conclusion, and the very great pains he took to endeavour to arrive at the truth;—how he made a journey with his able assistant and accomplished artist, Mr. Bauer, to Paris, ex-

* In Plate XIII, fig. 5, is represented the appearance of some of these entozoa, and of the particles which accompanied them in the fluid of the vesiculæ, collected in November 1837, preserved in dilute spirit, nearly a year and half.

† Vide Leuwenhoeck's letter to Lord Brouncker, dated Nov. 1677, in the 12th Vol. of the Philos. Trans., in which he mentions that the discovery of spermatic animalcules in man was first made by Ham, and how he confirmed it, and discovered them also somewhat different in form in the semen of the dog, cat, and rabbit.

pressly for the purpose of examining the spermatic fluid, with a microscope of Chevalier, by which the peculiar entozoa had been seen by certain French savans;—how he procured an excellent instrument of the same kind, and employed it at home; and how, aided by Mr. Bauer, after examining the spermatic fluid of the fallow-deer, when in heat, during two seasons, under most favourable circumstances, he was unable to discover any animalcules in it, and was confirmed in his opinion of their non-existence.

At present, this opinion of Sir Everard Home's being unquestionably erroneous, it is not necessary either to criticise or controvert it. The reflection, however, which it gives rise to, (if I may judge from my own feeling) is of a painful kind; especially in relation to his other microscopical observations,—so beautifully represented by Mr. Bauer, and admirably engraved,—in the accuracy of which now it seems a matter almost of impossibility to have any confidence. And this, I think it right to point out particularly, considering the manner in which Sir Everard Home prefaced the most remarkable of the microscopical observations,—observations which, if correct, would, as he anticipated, have been of the greatest importance, and which could not fail producing a great change in our physiological doctrines. Take, as an example, his introductory remarks to his Croonian Lecture, for 1817, "On the Changes the Blood undergoes in the act of Coagulation," in which he

endeavours to prove that blood-channels, giving rise to vessels, are formed in lymph by carbonic acid gas, in the act of passing, as it is disengaged, as he supposes, from blood in coagulating.

"It is not a little remarkable" (he says) "that in the first lecture of this kind, which I laid before the Society, in the year 1790, I should have endeavoured to show, that a muscular fibre was too minute an object to be seen by the human eye, even assisted by the best magnifying glasses then in use; and that, in this lecture, I shall be able, by means of the great improvements that have been made in the use of the microscope, to show that a fibre, not larger in diameter than one of the globules of the blood, can be demonstrated.

"To the members of this Society" (he continues) "who have so lately seen Mr. Bauer's drawings of the glandular apparatus peculiar to the Java swallow, of the internal membrane of the human stomach, exposing structures that were not known to exist; also of so small an object as the human ovum, in which is seen the seat of two of the most important organs of the body (drawings rendered beautiful by their simplicity and distinctness,) it will readily suggest itself, that Mr. Bauer is the person, to whom I consider we are indebted for those improvements. His whole life, I may say, has been employed in investigations of a similar nature, in plants, observing first, the natural appearances, and then magnifying them in different degrees, and comparing, with the

nicest discrimination, what was exhibited by one magnifying power, with what was shown by that immediately above it,—and, where they did not correspond, employing the whole energies of his mind, with a patient labour, almost beyond what is natural, in ascertaining the cause of the deception which must in one of them have taken place. To the observations of such a man upon subjects of this nature, if we are not confidently to place a reliance, how are we to give credit to the remarks that are made by common observers?

"I have said thus much" (he adds) "as an introduction to the observations that I am going to bring forward, for the public to know, whatever opinion they may form of them, they have been the results of long and unwearied research; and have been so frequently repeated as to satisfy Mr. Bauer of their correctness."*

The reader acquainted with the history of the microscope, and the powers of the best instruments which have been in use from the days of Leeuwenhoeck to the present time, will smile at Sir Everard Home's mistaken views of the improvement of the instrument as alluded to by him, and at the test which he gives of the power of the microscope employed by Mr. Bauer;—the defectiveness of whose instrument, it must be inferred, was one of the chief sources of error to which those two very zealous inquirers were exposed. I fear it must be admitted

* Philos. Trans. for 1818, p. 172.

of Sir Everard Home, that he was too ambitious of being ranked as a great discoverer, and that he had not the power of resisting the temptation of representing as a discovery, what he believed himself, although not clearly demonstrated. His account of the human ovum, referred to in the preceding quotation, may be noticed as an instance of this his disposition. It is now believed by those who had the best means of correct information on the subject, that the supposed ovum,—to which he attached so much importance, and of which there are such beautiful representations engraved in the Philosophical Transactions for 1817, from Mr. Bauer's drawings,—was only the egg of a large fly, deposited in the cavity of the uterus, after it had been laid open for examination.

Comparative Anatomy is under great obligations to Sir Everard Home; but, on that account, we ought not to take his authority for more than it is worth, and most of all be on our guard lest we are misled by one, who was extremely incautious and precipitate in forming his philosophical views. The Royal Society for the Promotion of Natural Knowledge has wisely always taken for its motto, the words

"NULLIUS IN VERBA."

XVIII.

OBSERVATIONS ON THE "AQUA BINELLI," WITH AN ACCOUNT OF SOME EXPERIMENTS MADE TO ELUCIDATE ITS SUPPOSED EFFECTS.

This water, for several years, has had some reputation in Italy, under the name of "Aqua Balsamica Arteriale," or the more brief one of "Aqua Binelli,"—the one designation derived from the properties attributed to it, the other from the individual who invented it.

The properties attributed to it by Binelli, and those who prepare and vend it at present, are not a little marvellous; such as the stopping both internal and external hemorrhages, and even of the large arteries when cut transversely,—the cleansing and healing of all kinds of wounds,—the renewal of uterine evacuations when suppressed, and the moderating of them when excessive,* &c.

* A pamphlet, descriptive of the effects of this water,—distributed with it by its vendors, thus opens:

"Da gran tempo è nota in Europa l' *Aqua Balsamica Arteriale* del dottor Fedele Binelli, nativo del Piemonte, e versatissimo in ogni

My attention was called to this water first by the late Captain the Honourable Sir Robert Spencer, in the summer of 1830, after a visit which he had made to Naples, at a time when the extraordinary virtues attributed to it were, in that city, a common topic of conversation, and when the proprietors of the secret of its preparation were selling it in large quantities.

The specimen which I examined shortly after was part of a case of several bottles which he had ordered to be forwarded to him at Malta,* for the purpose of having its qualities fairly investigated. It arrived after the decease of this enlightened officer, and was put into my hands for trial in our military hospitals, by the then governor, the late Major-General the Honourable Sir Frederick Ponsonby.

I first examined into its physical and chemical qualities. It proved to be of the same specific gravity nearly as distilled water. It was neither acid, alkaline, or saline. Its odour was not unlike that of coal gas not purified, which it lost by boiling; the taste was rather pungent; not in the slightest degree astringent. In brief, it appeared to be merely water containing a little volatile oil or naphtha, and

meditazione ed esperimento di chimica et di bottanica. L' efficacia della medesima ad arrestare ogni più minacciosa effusione di sangue, fu mai sempre si costante e di tal giovamento alla medica arte, che non potrebbe ottenersi più universale, e più meritata lode di quella che all' inventore di essa venne, da gran tempo, retributa."

* The charge for this case was about 8*l*. The price of the water at Naples, is four carlini, an ounce, or 2*s*. 8*d*.

was probably prepared by the distillation of water from petroleum or some kind of tar.

I next made trial of it as a styptic. I scratched the back of the hand with a lancet till the blood flowed; the water, applied to the scratch, rather increased the bleeding than stopped it. The following morning the razor inflicted a slight cut; the Aqua Binelli was again applied, and the result was the same.

These few and simple trials were made in January, 1831, just after I received the water, and they, of course, convinced me that the thing was an imposition on the public, and deserving of no further investigation. About two years after, my attention was recalled to the subject by a medical practitioner of Malta, who had studied at Naples, inviting me, with others, to witness the effects of a preparation made in imitation of the Aqua Binelli, and which, he maintained, was identical with it in composition and virtues.

The experiment he invited us to witness appeared an unobjectional one, namely, the partial division of the carotid artery of a goat, the bleeding of which he undertook to stop by means of his fluid. He allowed us to expose the vessel and cut it across; about one-half of the circumference of the artery was divided, and the bleeding was most profuse. He stood ready with compresses, moistened with the fluid, which he instantly applied, one over the other, and secured them by rolling a bandage about the

neck, making moderate pressure on the wounded vessel; a little oozing of blood followed, which soon ceased. He said that in three hours the bandage and compresses might be removed without any renewal of the hemorrhage.*

Accordingly, at the end of three hours, they were removed, but, when the last compress was raised, the bleeding broke out as furiously as at first, and, to save the life of the animal, the artery was secured by ligature. On examining the last compress, a small coagulum of blood was found adhering to it, just the size proper to close the wound in the carotid,—thus accounting for the ceasing and renewal of the bleeding.

Reflecting on this result, and considering the chemical nature of the fluid employed to moisten the compresses, which appeared analagous to that of Binelli, the conclusion I arrived at was obvious, namely, that had the compresses used been moistened simply with common water, the effect would have been the same,—the bleeding would have been

* The following marvellous result is related in the pamphlet already alluded to,—witnessed, it is asserted, by a commission composed of some of the most respectable medical men in Naples,—whose names are given, many of them professors in the university of that city. "Il Professore D. Nicola Mancini alla presenza della commissione, e di altri professori concorsi nel Teatro Anatomico del Regio Ospedale degl' Incurabili, apri trasversalmente l'arteria crurale di una pecora, vi pose sopra la filaccia bagnata nell' *Aqua Balsamica,* ed all' istante l'emorragia cessò; dopo un minuto fu tolta la filaccia, e l' arteria, con summa meraviglia degli astanti, si rinvenne già innestata, e la ferita netta come se mai da essa vi fosse sgorgato del sangue."

stopped; and it also appeared very probable that had the compresses been allowed to remain undisturbed there would have been no renewal of the bleeding.

To ascertain the truth of these inferences, the following experiments were made.

On the same day, February 8, 1833, in the presence of several medical officers, I divided partially transversely the carotid artery of two dogs; one, small and feeble, the other, of moderate size and strong. In each instance, the bleeding was most profuse, till compresses dipped in common water had been applied and secured by a bandage, which, as in the caseof the goat already given, completely stopped the hemorrhage.

The small dog, from the proportionally large quantity of blood which it lost, was very feeble immediately, and appeared to be dying; but it presently rallied, and for several days seemed to be doing well. It unexpectedly died on the 15th, seven days after the infliction of the wound. The bandage during this time had not been touched, and no application had been made. Now, on exposing the neck, the wound was found covered with coagulable lymph, discharging pus, and on dissecting out the artery and the eighth nerve contiguous to it, a mass of coagulable lymph appeared lying over the wound in the vessel, extending about half an inch above and below it. This mass of coagulable lymph having been carefully removed, and the artery slit open, the vessel

was found quite pervious, not in the least contracted. The wound in the fibro-cellular tissue or external coat was closed by a minute portion of dense coagulable lymph. But not so in the middle and inner coat; in these there was a gaping aperture, across which on minute inspection two fine threads apparently of coagulable lymph, as if the commencement of the healing process, were observable (vide Plate XIII, fig. 6). The cause of the dog's death was not discovered.

The other dog did not appear to suffer from the wound. The bandage and compresses were removed on the 15th February, without the occurrence of any bleeding. On the 20th of the same month, the wound in the neck was nearly closed by granulations. The artery was now cut down on, and the portion of it that had been wounded taken out between two ligatures previously applied. On careful examination of this excised part, it was found free from coagulable lymph—at least there was not the same thickening or tumour from lymph deposited as in the former case; it was probably absorbed. When the external loose cellular tissue was dissected off, a very minute elevation, about the size of a pin's head, appeared on the site of the wound, the remains of the cicatrix externally. The artery was completely pervious, and not at all contracted, where it had been wounded. Slit open for internal examination, the wound in the inner coat was marked by a red line interrupted by two white spots; there was no gaping,

the edges adhered together, excepting at one point, elsewhere the union was complete. The white spots resembled the natural lining membrane, and had the whole wound been similarly healed, I believe it would have been impossible to have traced it (vide Plate XIII, fig. 7).

These experiments were made in Malta. I shall mention one more, which was made after leaving that island, and subsequently to the publication of an account of them in the Edinburgh Medical and Surgical Journal.

On the 29th November, 1837, at Fort Pitt, the carotid artery of a pointer was laid bare, and partially divided transversely. Very florid blood gushed out with great violence. Two linen compresses dipped in water were immediately applied one over the other, and secured by a roller. It was drawn too tight by an assistant, and the hemorrhage was not suppressed, until the pressure was relaxed, indeed it was necessary to remove the bandage, and reapply it, *not tight.* About six ounces of blood were lost during the operation. Nothing farther was done; there was no farther bleeding: the dog's health was not affected. In about a fortnight, the wound in the integuments had completely healed; and in about six weeks, namely, on the 12th of January, on examining the artery carefully dissected out after the death of the animal, from the injection of pus into its pleura, no traces of the wound could be detected in the outer

coat of the vessel, or in the contiguous parts, nor internally when slit open, excepting on narrow scrutiny, when a faint line was just perceptible, marking the cicatrix, like the impression made by a fine ligature on an artery not drawn sufficiently tight to divide the inner and middle coats (vide Plate XIII, fig. 8).

The general results of these experiments, (if I may be allowed to speak so of so small a number) are not without interest in application to surgery. They show how a hemorrhage from the wound of a large artery, which by itself would be speedily fatal, may be easily arrested by moderate compression through the means merely of several folds of linen or cotton moistened with water; and they further shew how, under this moderate compression, the wound in the artery heals, the vessel remains pervious, and without the formation of an aneurism, and how, after a time, the short space of six weeks, only just perceptible, traces of the wound are discoverable. Under this moderate compression, the healing of the wounded artery seems to be very analogous to that of a wounded vein, and apparently by means of the same natural process.

Whether similar results would be obtained were trial made of the same means in the wounds of arteries in the human subject, can only be ascertained positively by judicious experiments. The probability is, the results would be the same; the analogy is very complete, and some facts well known

in surgery accord with it, not to mention the experience of the effects of the aqua Binelli, as certified by men of high respectability.*

I have laid stress on the effect of the *pressure*, afforded by the wet compresses applied in the experiments related, believing that the virtue of the means consists in the pressure,—of course not in the water, excepting so far as it renders the compresses better fitted for adaptation to the wound to produce the degree of resistance requisite to counteract the heart's impulse on the vessel; and also better fitted to exclude atmospherical air. I would also lay stress on the *moderate* degree of pressure that is produced in the manner described, allowing the blood to pass through the canal of the artery, and as before observed, doing little more than resisting the momentum of the blood in its passage from the moving source. The importance of this moderate degree of pressure,—reducing as much as possible the wounded artery to the condition of a wounded vein, is, if I do not deceive myself, very

* It was my intention to have given a selection of the certified cases, in favour of the aqua Binelli brought forward in the pamphlet which is furnished with the water; but on reconsidering them, it appeared a superfluous labour, as the results (giving them credit for correctness) however excellent in a curative point of view, are no more than the enlightened surgeon of the present time may readily admit to be owing to water dressings alone without the aid of pressure; the majority of the instances adduced, being examples of gunshot wounds, and contused wounds, from which there was no profuse bleeding, and no necessity, according to the ordinary mode of surgical treatment, for securing wounded vessels.

considerable. When I have pressed with the fingers forcibly on the compresses applied to the wound, expecting at the moment to arrest the bleeding, I have been disappointed,—it has continued. It has only ceased when the compresses have been secured, and not tightly, by a roller passed round the neck of the animal. And further, in illustration, I may remark that I have been equally disappointed in using graduated compresses, ensuring considerable pressure on the wound. This method has failed, when general moderate pressure effected by compresses about two inches long, and one wide, succeeded. And, on reflection, the difference of result is perhaps what might be expected; the severe pressure can hardly arrest the bleeding except by pressing the sides of the vessel together, and closing the canal, the accomplishment of which requires a most nice adaptation, and a force, which cannot easily be applied with steadiness except by mechanical means, and in situations affording firm support beneath.

Should the expectation which I have ventured to form of this method of stopping the bleeding of wounded arteries of a large size, in man, be realized on trial, I need not point out how very useful it may prove in military surgery, how very available it will be in the field and in battle, especially in great actions, when however numerous and well appointed the medical staff of an army, the number of wounds requiring attention must always exceed the means

of affording adequate surgical relief, according to the plan of treating them at present in use, of suppressing hemorrhage by ligature.

I have said nothing of the boasted efficacy of the aqua Binelli given internally. I trust it is as little necessary to make any comment on it now-a-days, as on the tar-water of Bishop Berkeley, so very analogous in nature and reputation. Both the one and the other in some cases may be serviceable, but their principal recommendation seems to be, that in doubtful cases they are innocent.

XIX.

ON A NEW METHOD OF PRESERVING ANATOMICAL PREPARATIONS FOR A LIMITED TIME.

During two years and half, namely, between 1825 and the latter end of 1827, as leisure and opportunities permitted, I first made trial of a method of preserving anatomical preparations, which, I believe, to be new, and which I have found to possess advantages, in many respects, greatly exceeding the expectations I had formed of it.

Its principal advantages consist, — first, in its cheapness; secondly, in its durability within certain limits; and, thirdly, in the clear and instructive manner in which it displays the minute structure of many textures and compound parts.

The cheapness of the method will be obvious, merely from the mention that the substance employed is a solution of the sulphurous acid gas in water, and that it may be prepared in a manner, equally economical and easy, by burning sulphur over distilled or rain water, in any appropriate glass vessel;*

* A tall receiver, provided with a cork, one-third full of water,

agitating the water, when the sulphur ceases to burn, and, when the water is sufficiently impregnated with the acid gas, filtering the solution, to render it transparent and clear.

Its durability, to a certain extent, I infer from many circumstances. In December, 1827, when I communicated an account of the effects of the acid to the Medico-Chirurgical Society of Edinburgh, in the third volume of whose Transactions it was published, I had preparations by me, which had then been made nearly three years,—nothing further had been done to them,—no fluid had been added,—no evaporation had taken place, and they then appeared as perfect as when they were first immersed in the acid, and merely confined in a bottle with a glass stopper, lubricated with a little cerate. Other considerations induced me to think favourably of its preservative powers. From experiments I had previously made, the sulphurous acid appeared to have nearly the same power in preventing the putrefaction of animal matter, as it has of stopping the fermenta-

answers well. The sulphur may be introduced kindled, in a small gallipot, attached to a strong wire. As soon as the flame expires the sulphur should be quickly withdrawn, the cork replaced, and the vessel agitated, that the water may absorb the acid gas formed. After agitation for about half a minute, the cork should be taken out,—and fresh atmospheric air supplied by blowing into the jar with a common bellows; the burning of the sulphur may then be repeated, and the process so continued, until the water is sufficiently strongly impregnated. Hard water should be avoided; for, if the water contain any lime, it will render the solution turbid,—a fault which even filtering will not perfectly correct.

tion of vegetable juices. I found that serum coagulated by this acid gas, and converted into a kind of jelly, may be kept in water several weeks, exposed to the air, without undergoing any change, and that the fibrin of the blood, thus treated, was equally exempt from change. Similar trials were made with many anatomical preparations; they were exposed to the air in this acid, and were then taken out of the acid and placed in water exposed to the air; and the result shewed that after having been well acted on by the acid, they were no longer liable to putrefaction. Some of them underwent very little change, during exposure to the air, for several weeks in water; others became soft and gelatinous, and were partially reduced to a pulp, but without emitting any offensive odour, like that of putrid animal matter, an odour rather like that of decaying vegetable matter affected with mildew.*

How the sulphurous acid acts in preserving animal substances it is difficult to say. I am not aware that it is yet ascertained how it preserves vegetable substances; how it prevents vinous fermentation in vegetable juices, such as are most prone to undergo it, as the juice of the grape, &c.; how it prevents light wines and vinous fluids to which atmospheric air has access, from becoming sour or converted into vinegar; or how, moreover, it stops the process of ulterior change to which unpurified vinegar, and

* These experiments were made at Corfu, and during the hot summer months.

most, if not all, of the unpurified vegetable acids are liable, when exposed to the air. It is not owing to the mere abstraction of oxygen, for phosphorus has not the same effect permanently, nor nitrous gas. It may be conjectured, both in the instance of vegetable and animal substances, that the antiseptic power of the acid depends on its effecting some new arrangements of the particles of the proximate principles which render them exempt from the changes alluded to; or on rendering inert the vegetable and animal leaven which, under favourable circumstances, excite these changes.

The last advantage which I have mentioned, the manner in which the acid displays the minute structure of many textures and compound parts, is that which I shall most dwell on, as I consider it its chief recommendation to notice. It does not, like spirits of wine and a solution of alum, contract what is immersed in it; it does not, like a saturated solution of common salt, or of nitre, or of any of the salts of chlorine which I have tried, after a little while, lose its transparency and become thick and turbid; nor does it, like a solution of corrosive sublimate, when used without precaution, deposit on the inside of the glass and on the preparation itself, a crust which soon becomes a complete mask. On the contrary, it expands and developes the parts, some more, some less, so as to magnify them and make them more distinct, effecting in structure what the lens does in vision; and, at the same time, it remains clear, so

that the lens still may be employed to heighten the effect and convey still more minute information of the object.

I shall enter into some details in illustration. All the descriptions will be taken from preparations from the human subject.

The skin immersed in the sulphurous acid swells considerably, and the cuticle is either thrown off or easily detached. The cuticle of the sole of the foot, to mention a particular instance, is rendered almost transparent, and is but little thickened. When held between the eye and the light, its symmetrical structure is beautifully shewn, with innumerable dotted points, as it were, in the course of its linear waving, which probably serve the purpose of pores. If the cuticle, detached by means of sulphurous acid, is dried, it shews its peculiar structure still more distinctly and elegantly.

The cutis, distended by the sulphurous acid, appears as a tissue of extremely minute fibres and particles, condensed towards the outer surface, where in contact with the cuticle, and loose internally, where it gradually blends itself with the adipose texture.*

Serous membranes, immersed in the acid, swell considerably, and lose very little of their transparency. The inner surface of a portion of pericardium,

* In the scrotum and eye-lids,—the transition is into cellular texture; the surface of the cutis appears as the boundary merely of the cellular tissue a little condensed.

now before me, has an uneven surface, and exhibits when examined with a common lens, a slight appearance of pores; its section shews as if it were composed of layers, and its outer surface displays a loose intertexture of fine fibres. A portion of pleura, (with the exception of the cut part, which is very much thinner, and not visibly in layers), appears very similar, but less distinct.

On the cellular structure, the sulphurous acid has much the same effect as on the serous;—it distends it greatly, and preserves it transparent.

On fibrous membranes the sulphurous acid has not much effect; they become swollen in it slightly, and diaphanous. The outer layer of the dura mater, in a specimen before me, is considerably corrugated, whilst the inner has remained smooth, which, perhaps, is owing to the outer being of greater density than the inner, (as it appears to the eye to be); in consequence of which greater density, it is more expanded. The sclerotic coat of the eye is equally acted on throughout; and is not in the slightest degree wrinkled. The outer surface of the tunica albuginea testis is corrugated very slightly.

It acts more powerfully on tendons and ligaments; and on some more than on others, shewing a striking difference in this respect. The tendons of muscles in general it expands amazingly, and renders semi-transparent. To take an example,—the tendo achillis thus swollen has the appearance of an opalescent mass, divided pretty regularly into cells, the

boundaries of which are white opaque lines. On the ligamentous sheath of the penis its expansive power is less forcibly exerted; the sheath is rendered nearly transparent; and its structure is beautifully shewn, as if composed of a white pulp as a groundwork, intersected and bound together by very fine opalescent fibres. On the aponeurotic sheaths of muscles, its effects are very similar. On the chordæ tendineæ of the heart its effect is inconsiderable; it expands them but little, and renders them semi-transparent. On one part of the intervertebral substance it acts in the same manner as on tendon, of which part the inner portion, that nearest the cavity of the abdomen, chiefly consists, at least in the lumbar region; but, on the other part, its action is hardly perceptible. In consequence of this difference of action of the acid on the component parts of the intervertebral substance, it is well adapted to display its peculiar compound structure. When the acid is strong, an imperfect solution of a part of the tendon or ligament takes place, which subsides in the form of a semi-transparent very fine jelly.

Cartilage is not changed by the acid; neither the dense cartilages of the ribs, nor the delicate ones attached to certain organs, as the tarsus, epiglottis, &c. It is equally inactive on bone. Nor does it appear to have any sensible effect either on muscular fibre, or on the substance of the brain and spinal chord, or of the nerves and ganglia.

On mucous membranes its action is considerable:

it does not appear to dissolve them in the slightest degree, but it distends them; renders them firm; and shews their peculiar structure in a striking manner. As an example, I shall give a brief sketch of the primæ viæ.

First, I may premise, that it displays the cuticle or epithelium, terminating abruptly in the œsophagus, just above the entrance of the latter into the stomach, and in the rectum, just within the circle of the anus, or the upper boundary of the sphincter muscle. In both, the termination is distinct, and abrupt, and unquestionable, as it appears in a preparation now before me.*

When the epithelium is removed, (and it is most easily, either by placing it under a stream of water, or by the friction of the fingers), and the mucous coat is brought distinctly into view, in the upper part of the œsophagus it appears very thin, provided with many and pretty large mucous follicles on each side, and several minute branches of nerves. With a magnifying glass of ordinary power, it seems to contain a most delicate tissue of white lines, running chiefly longitudinally, branching off slightly

* It appears very questionable, that the stomach and intestines generally, the gall-bladder and the biliary ducts, the urinary, seminal and the air passages, possess an epithelium analogous to cuticle and a continuation of it. All these parts must necessarily have a lining membrane, which seems to be their mucous coat, in each part somewhat different. It has been supposed that the mucus secreted by the membrane serves in lieu of cuticle, and it seems to be a probable conjecture.

and anastomosing. The same appearance presents itself in the middle and lower part of the œsophagus, and more distinctly. But it is only in the upper part that any nerves can be seen distinctly ramifying in the semi-transparent mucous tissue. In the middle no follicles are visible, and only a few near its termination. Where it terminates, there is, in one preparation before me, quite a zone of what appears to be minute follicles: but this does not shew itself in another preparation.

At the cardiac orifice of the stomach many minute follicles present themselves. Where the gullet ends, and the stomach commences, there is a sudden change of the appearance of the mucous texture; the linear marking suddenly ends; and, as it were, a dotted one commences, and with some variations, extends throughout this organ. When magnified, it has the appearance of a very delicate lacework, formed in the upper part, by the close juxta-position of circular lines; and, in the lower, by lines in the form nearly of the figure of 8; and, in the great arch of the stomach, by lines irregularly tortuous. But this, it must be confessed, is not to be seen in every preparation that I have examined; and, therefore, I would rather limit myself by saying, that, in the stomach, the mucous texture has the appearance of a very delicate lacework of lines or vessels.

At the commencement of the duodenum, the appearance of the texture again distinctly changes. Here it appears as if covered with tortuous threads

or vessels twice or thrice the size of those of the mucous coat of the stomach; and, as the duodenum descends, the appearance strengthens. In some preparations there is an appearance of follicles, and of circular depressions, but not in all I have examined; and, when seen, they are most distinctly seen with the naked eye. A longitudinal section, including the pylorus, strikingly and invariably shews a layer of a minute glandular structure,* embracing and belonging to the mucous coat of the duodenum, which terminates abruptly at the pylorus, but gradually in the contrary direction, and disappearing, or nearly so, about an inch and a-half from the pylorus. The same section, too, shews how very thick the muscular and cellular coats of the stomach are, in comparison with those of the duodenum.

The inner coat of the jejunum, viewed with the naked eye, in sulphurous acid, appears to be distinctly villous, or as if studded with innumerable projecting capillary points; but this appearance vanishes when it is examined with a common lens; then, in some lights, it appears to be covered with convoluted threads, having a kind of centre, round which they are described, which, to the unaided eye, seems a minute mucous gland; and, in other lights, as minute projecting laminæ variously bent.

* The glands of Brunner, first described in 1687, and called by him, not inaptly, *pancreas secundarium*.

As the jejunum passes into the ileum, and as the ileum descends, another change takes place, and it is very strongly marked close to the termination of the latter intestine, where the mucous coat is truly villous;—so it appears to the naked eye in sulphurous acid; and, when examined in a favourable light with a lens, the villi exhibit the appearance of tubular projections rounded at the ends; and some of them have a form approaching to the conical, rather than the cylindrical. The sulphurous acid makes the glandular structure* of the lower part of the ileum very distinct to the naked eye.

This villous structure terminates suddenly at the valve of the colon (where the valve begins), and a new kind of structure succeeds it in the valve, and proceeds, with very little variation, of the same kind throughout the large intestines, even to their termination, not excepting the appendicula vermiformis. Its resembles honey-comb in its appearance, more than any thing else to which I can compare it; but it is so very minute and delicate in every part of the large intestines, that it cannot be well distinguished, excepting with the aid of a magnifying glass. The glands, on the contrary, of the large intestines, appear very distinct in the sulphurous acid; and their orifices are easily seen.

* Glandulæ aggregatæ; Peyer's glands, not discovered but minutely described by him in 1677.

Relative to the other mucous membranes, I may briefly mention, that this acid shews them equally as well as the primæ viæ, and enables one equally well to notice how they differ from each other.

I shall now proceed to some other parts. By rendering the fibrous neurilema, and the connecting cellular tissue, semi-transparent, and by expanding these textures, the sulphurous acid has a remarkable effect in displaying the nerves, with the exception of the ganglionic. It shews, almost without dissection, and still better when aided by dissection, which it facilitates, the parts of each fasciculus of nerves and their junctions; and, if the nerve be cut, the proportion of the medullary matter which it contains, which becomes projecting, squeezed out, owing to the pressure just alluded to. It shews very distinctly the fibrous nature of the optic nerves beyond their junction; but, at their junction, and towards their thalami, their substance appears to be exactly similar to the common medullary substance of the cerebrum. It shews the papillæ of the tongue to be each the termination of a minute nerve, and this without the aid of a magnifying glass. It demonstrates how, as the nerves proceed from their source, the proportion of medullary matter diminishes, and the thickness of their sheath increases; and this very remarkably in the nerves of the fingers. On the ganglionic nerves, judging from the very few trials which I have yet made, it appears to have but little effect, as if their composition were different

from that of the other nerves, as has been lately maintained.

It displays, too, almost without dissection, the composition of the vessels, and, I may add, of the passages and canals of the body generally; owing to its expanding, in different degrees, the several coats of which they consist.

The manner in which it exhibits the muscles is most distinct in all the details of their anatomical structure. A transverse section of a long muscle, well steeped in strong sulphurous acid, is an interesting anatomical object. With the naked eye its constituent parts may be clearly seen, the blood-vessels and nervous filaments, the muscular fibres collected in bundles of different sizes, and connected by cellular tissue and ligamentous threads.

The effect of the acid on other compound parts is not less instructive. Were I to describe the appearance which the more important only exhibit in this fluid, I should greatly exceed the limits to which it appears desirable to confine the description. In this part of my subject I shall restrict myself to one example, which I select, as being well adapted to illustrate the effects of the acid on an organ variously compounded. This example is the penis: immersed in the acid, it swells like a sponge when put into water, and very soon becomes completely distended; then, when the cutis has been dissected off, its ligamentous sheath is brought beautifully into view, with the nerves and vessels on the dorsum penis running

superficially through this sheath, now rendered transparent; and sections, in different directions of the organ, display equally clearly its internal structure, which, to the eye, either naked or assisted by glasses of different powers, appears to be distinctly cellular, analogous to the substance of the lungs; or, perhaps, still more analogous in appearance to the structure of the common sponge, especially the anterior part of the corpora cavernosa, where the cells irregularly formed are largest, and where, as in a cut piece of common sponge, there are large circular orifices, which seem to belong to canals formed of ligamentous substance, which penetrate a certain way, and terminate abruptly with closed extremities.*

From such trials as I have yet made of the effects of sulphurous acid on the textures of other animals besides those of man, I am of opinion that it may be usefully employed in comparative anatomy, especially to aid in the display of minute structure. For this purpose, inquirers will find it very serviceable, in the instances of preparations sent from abroad unskilfully put up, or in examining the viscera of animals which have been obtained entire, preserved in spirits. The effect of the acid in dilating the contracted parts, especially those which are membranous and cellular, as the primæ viæ, and the air passages and lungs, exceeds expectation. If the parts are delicate, I should mention by way of pre-

* These, probably are the arteriæ helicinæ of Professor Müller.

caution, that the acid is not fitted for preserving them. When distended, they should be transferred to dilute spirit of wine. If allowed to remain in the acid, they may become softened and in a few months reduced to the state nearly of a pulp: this I have witnessed, in the respiratory and alimentary passages of small birds.

For the purposes of pathological anatomy, I believe the sulphurous acid will prove equally if not more useful, than for those already alluded to. I rest this opinion partly on the trials which I have made, and partly on the consideration, that, whilst its expansive power is so great on most tissues, its solvent power is very inconsiderable, and on coagulable lymph appears to be null, (an ingredient of the blood, which, in most morbid changes, acts so important a part); and it appears to be null on mucous membranes, the seat of so many diseases. The specimens which I have by me, of diseased parts preserved in this acid, are satisfactory beyond my expectation; not so much as shewing the exact diseased appearance, such as it presented itself on dissection, as in giving a correct notion of the nature of the lesion, by magnifying and rendering more distinct the effects of the disease, contrary to what happens when the morbid parts are immersed in spirits of wine.*

* For the purpose of pathological research, the part to be examined may, with advantage, be immersed for a few hours in sulphurous acid, and then transferred to a cylindrical bottle, of thin and very clear

The sulphurous acid enables one to detect the slightest ulceration of mucous membranes, existing even in so inconsiderable a degree, that, without this aid, it might escape detection. It enables one, too, to detect the cicatrices of old ulcers;* to detect coagulable lymph poured out and adhering to their surface, even in the intestines, constituting a false membrane; and to notice many other changes in relation to the different coats, vessels, &c. which it would be tedious to enumerate.

It shews equally well the effects of disease on serous membrane, on the blood-vessels, and on the viscera, especially the lungs. I have, in my possession, preserved in this acid, a very delicate specimen of tubercles on a portion of pleura costalis, covered with a very fine net-work of coagulable lymph, and, after several months, it is as distinct as when first immersed, and more distinct than before it was introduced into the acid. A portion of hepatised lung, thus preserved, shews the air-cells filled with an albuminous deposition, and the blood-vessels closed with the same, and the minute branches of the bronchia. And a vomica, which has been kept in this

glass, full of water, perfectly clear; very slight changes in the delicate structure of the part, before obscure, or not to be distinguished, will become apparent and distinct; and more particularly, very slight lesions and changes, whether recent or old, of the mucous membranes.

* These cicatrices, from the observations which I have made, seem to differ chiefly from sound mucous membrane, in containing no follicles, when the previous ulceration has been deep; and, when only superficial,i n exhibiting a structure more or less different from that of the parti n health.

acid, displays not only the ordinary phenomena of a tubercula rexcavation, but, what is considered a very rare occurrence, blood-vessels (veins) terminating in the cavity with open mouths.*

I apprehend it will prove very useful in studying the nature of tubercles, and in determining some doubtful points respecting them. There are two preparations now before me, which I may mention in illustration; one, a portion of lung containing tubercles in their early granular stage; another, containing a tubercular mass: the former are clearly in the cellular structure of the organ; in the latter blood-vessels and bronchial tubes may be detected, clearly indicating that the tubercular matter has been deposited on them so as to include them,† according to the analogy of the deposition of lymph in hepatized lung.

The morbid changes of the arteries are very easily examined and exposed by immersion in this acid, and chiefly in consequence of its rendering the different coats so very distinct and distinguishable. In all the specimens of aneurism which I have examined,

* This occurrence, I believe, is much less rare than is commonly supposed. It may frequently be detected in tubercular excavations, by passing fine probes into the vessels which terminate in the cavity abruptly, marked by a short projecting portion, their course having been interrupted by the ulcerative process which formed the vomica. I have often found the canal of the vessel unclosed, its aperture, in a manner, hid by the projecting portion, which, in some instances, has been so situated as to act the part of a valve.

† The appearance mentioned above, not carefully examined, might give rise to the idea, that tubercles are vascular.

and in every instance of tendency to it, I have found the inner and middle coat of the vessel diseased; the latter generally diminished in thickness, and rendered brittle, and sometimes entirely absorbed in the dilated part of a true aneurism, or in the part contiguous to the orifice communicating with the sac of the false; and the inner coat corresponding generally thickened, and corrugated, and easily broken. In many instances, since I have used the sulphurous acid, I have noticed a very strong tendency in the arteries to aneurism, with only incipient dilatation, in which the middle coat of the vessel has been partially reduced to the thinness of the finest paper, and its colour changed to yellow; and the inner membrane admitting of being easily detached, has been thickened to thrice or more its thickness in its sound state.

Lastly, in relation to the use of sulphurous acid for anatomical purposes, I must not conclude without offering some precautions taught by experience, the neglect of which may occasion want of success.

1. It is of consequence that the part to be preserved should be immersed in the acid, as soon as possible; for if in the slightest degree putrid, it will not be well preserved, the fluid will become turbid, though frequently changed, and the preparation will not keep.

2. If the part be putrid, as is often the case, when taken from the body, though the *post-mortem* exami-

nation be made a few hours after death, it should be immersed in a solution of chlorine in salt and water, till deprived of its putrid smell and tendency; then it may be washed clean, and put into the acid solution, without danger of spoiling.

3. Attention should be paid to the strength of the sulphurous solution, to proportion it to the nature of the part to be preserved, and the object in view in employing it. If the intention is to expand and develope the structure of the parts, for the purpose of demonstration, a strong solution answers best; if merely to preserve a part as little changed as possible, a weak solution is most successful, especially if cuticle, the visible epithelium, or tendinous parts, are concerned, as a strong acid separates the former, and partially dissolves the latter.

When strong acid is thus used, if it be desirable to keep the part, the structure of which has been distended, it is safest to transfer it from the acid to dilute spirit of wine, as proof spirit diluted with twice its bulk of distilled water.

4. If the object is to preserve a part for a number of years, I would recommend in every instance the transferring it from the acid to dilute spirit, such as that just mentioned; especially if the preparation be scarce, and of value in consequence. I am induced to offer this suggestion from the experience which I have had during the last twelve years: during this period some preparations have been well pre-

served; others have undergone change, and have become spoilt. The uncertainty of preservative effect is sufficient ground for caution.

I believe, too, that this acid is not inapplicable to the service of the botanist, for preserving entire plants and their parts, especially the smaller and more delicate. I have a bottle now before me in which many kinds of vegetables are immersed, and have been kept several months, and their forms are as distinct as ever, and in excellent preservation; and amongst them I see the flower of the olive and the orange trees, and many more equally delicate.*

* The effect of the sulphurous acid on the colour of vegetables, and I may add of animal substances, is much less than might be expected. Some vegetable colours it does not change, others but slightly, and very few entirely; and even the last-mentioned return, though not with their original intensity, when the sulphurous acid is converted by the absorption of oxygen gas into the sulphuric. The red rose, bleached white by the fumes of burning sulphur, on exposure to the air, again becomes red. Of animal textures, those which are white, it renders delicately so; those which are yellow, as the gall-bladder, and common gall-duct stained by bile, the liver of this colour owing to disease, and parts of the supra-renal gland, it does not alter. It has very little effect in altering the healthy colour of the lungs or liver; but, where there is blood either effused in the parts, or contained in its vessels, the acid changes its colour to dark-brown, and, by coagulating it, fixes it for a time; and thus it is well adapted to detect the effusion of this fluid, in parts where it is apt to escape observation, as in the substance of the spleen, liver, &c. It is often difficult to distinguish between the redness produced by inflammation, and that which is occasioned after death by the staining power of the blood. I believe (and not hypothetically) that the sulphurous acid affords a means of distinction. If the change should have taken place after death, the part stained will be rendered brown by the acid, and in other respects appear sound; but if, during life, owing to in-

On the economical uses of this acid, and they are many, as in relation to its power of preventing the fermentation to which all the saccharine preparations of the kitchen are so subject,—of preventing the changing of wines into vinegar, to which all the lighter and more delicate ones are liable,—and, further, of preventing the common vegetable acids, as vinegar and lime juice, &c., and vegetable preparations in general, from spoiling, it would be unsuitable in this place to dwell. I shall merely remark, that the efficacy of the acid in all these respects is great and extraordinary, as must be known in part to many, and which may be learned by the easiest trials.* This subject, the economical uses of the sulphurous acid, is an important one to society, and would well reward the labours of the inquirer who would undertake the minute investigation of it.

flammation, by immersion in the acid, either coagulable lymph will be detected effused on the surface, or under it, or serum in the adjoining cellular tissue, or some slight ulceration of the part affected.

* It is not improbable, but that it may be useful for the preservation of meat, especially in a hot climate. I recollect, when sanguine of its efficacy, at the time I was engaged in experiments on it, keeping a chicken a week in the acid gas in the hottest time of the summer of the Ionian Islands, and having it approved of at table.

XX.

ON THE ACTION OF VINEGAR ON ANIMAL TEXTURES.

Although vinegar, of all the acids, has been longest known, and from time immemorial used as a condiment, I am not acquainted with any account hitherto published of its effects on animal matter, minutely considered, or of its application to anatomical purposes. With the desire of obtaining such information, several years ago I instituted some experiments on the subject, the results of which I shall now record, believing that they may prove not without use or devoid of interest.

The vinegar I employed in the inquiry was distilled vinegar, almost colourless, of specific gravity 1006. This kind appeared preferable, both as belonging to the materia medica, and being nearly pure and almost always of the same strength. On the contrary, the common kinds of vinegar, those which have not been subjected to the process of distillation, are extremely various in composition; and, according to this variation,—according to the nature

of the vegetable principles which they hold in solution, they are liable to vary in the changes which they themselves undergo, and in their effects on the animal textures immersed in them.

It might be expected, perhaps, that the solvent power of vinegar, on the textures of the body, would be considerable; but, experiment proves it to be otherwise. In 1828, whilst stationed in Malta, portions of aorta, dura mater, brain, nerve, muscle, intestine, skin, liver, and intervertebral substance, were put into this acid, in a vessel not completely filled, and from which atmospheric air was not completely excluded. This was done in the month of January; at the end of six months, all the parts were in good preservation. The acid retained its transparency, but was slightly coloured, and there was a slight sediment. I may mention farther, to shew how very inconsiderable is the solvent power of this acid, that on evaporation, it afforded only a very minute residue, of a light brown colour, semi-transparent, viscid and bitter: the greater part of which was soluble in water.

The colourless acid produces very little change of colour in any of the textures on which I have tried it,—unless the parts were gorged with blood; and then the alteration is referrible to the blood, the colouring matter of which, when not concentrated, is changed to brownish green; and, when concentrated, to almost black.

To ascertain the degree of dilution, which the

acid, of the specific gravity already mentioned, will bear, without being deprived of its antiseptic power, I have used it diluted with one, two, four, and eight parts of water. Portions of muscle were immersed in these mixtures of vinegar and water, and air was allowed to have access. Examined after two months, during which the atmospheric temperature varied between 80° and 92°, each portion of muscle was found well preserved and free from putrid smell, excepting that in the most dilute,—which, in a few days, began to putrefy: the experiment was made in Malta. In this country I have made a few other trials. In November, 1836, a portion of stomach, of duodenum, ileum, rectum, and tongue, after having been steeped for a day or two in distilled vinegar, were transferred to weaker acid, (distilled vinegar diluted with four parts of water). Three years have now passed, and all these parts are in perfect preservation. Each was placed in a jar apart, suspended in the fluid in the usual manner, and covered with bladder, &c., to exclude atmospheric air; and, with the exception of one, after the lapse of the time mentioned, no air has got admission. In the exception, the bladder was attacked by insects, and yet, notwithstanding the accident, and partial evaporation and free admission of atmospheric air, the preparation (the tongue) is in a good condition and displays well the peculiarities of the organ.

Vinegar, I find, has not only the power of preventing putrefaction from taking place, but also of

arresting it, when commenced and in full activity. So soon as the putrefying part is plunged into the acid, the destructive change is stopped, and the part may be preserved without farther change; and thus shew the effect of putrefaction on the texture in the stage in which it was arrested.

But, although the acid stops putrefaction, it does not, like the sulphurous acid and corrosive sublimate, destroy the principle on which the change depends, or effect an elementary change, incompatible with its taking place. I find that parts transferred from acid to water, and well washed, undergo putrefaction under favourable circumstances,—but much more slowly than if they had never been in contact with the acid: and, the changes which take place in them, seem to be, in some respects, different from those resulting from ordinary putrefaction, and are, perhaps, peculiar.

In its action on different textures, in relation to its visible effects, it very much resembles the sulphurous acid. Its expansive power is similar; its power of increasing the transparency of cellular tissue and of the diaphanous membranes is also similar. And, in consequence, like that acid, it is well fitted to shew the structure of many compound parts, and the minute structure of many organs. I have mentioned preparations of different portions of the alimentary canal, preserved for two years in dilute acid. Without the aid of a lens, the peculiar structure of each division may now be distinctly

seen, and the mucous follicles even more distinctly than in the instance of sulphurous acid,—in consequence of the glands themselves having acquired a certain degree of opacity, and the enveloping tissue a certain degree of transparency. I have now before me a thoracic duct, which has been kept in distilled vinegar nine years, and it could not be shewn better were it injected with mercury; with this advantage on the part of the acid,—that, whilst by distension it shews the form of the vessel, it also, where the vessel is laid open, shews its valvular structure.

From my own experience, I believe I am not too sanguine in expressing, confidently, my opinion, that both the anatomist and pathologist, engaged in research, may derive very material help from the use of this acid, and the latter, especially, as it is well fitted to bring to light diseased structure, of such minuteness or obscurity, as to escape detection by the senses unaided.*

There are other uses to which this acid is applicable, some of which, as connected with my present subject, I shall briefly advert to.

On account of its antiseptic quality, I believe, distilled vinegar, may be often usefully employed in surgery, in washing foul ulcers and in washing out abscesses deeply seated, containing offensive and

* Minute ulcers in the intestines and loss of the villous structure, I have several times detected and made apparent by immersion in this acid, not perceptible in ordinary examination.

putrefying matter. In one instance, of an abscess in the liver, which was opened by the knife, this acid, diluted with water, was employed, at my recommendation, and apparently with very good effect; for, although air got admission, putridity was prevented.

Whether vinegar is efficacious, as a means of fumigation, for which purpose it is still commonly employed, it is not easy to decide. Its properties, on the whole, are rather unfavourable to its efficacy,—at least permanently. However, as it insures, by the pungency of its fumes thorough ventilation,—as it is most easily applied,—has no noxious quality that is known;—and, its activity, when concentrated, is certain and considerable,—perhaps it deserves the good opinion it has gained, and it may be right to continue the employment of it, as at present, when slight fumigation only is required, and there is no suspicion of the presence of any pestiferous fomes.

As vinegar dissolves tannin with facility, and in considerable quantity, it may be added, with good effect, in tanning such parts of the body, as it may be thought desirable, by this process, to convert into dry preparations. The addition of the acid insures the advantage of preventing putrefaction,—and the consequent destruction of delicate organization. I have by me, thus tanned, the stomach with part of the œsophagus, its épithelium adhering, the valve of the colon and some other parts of the intestines. They display accurately, not only the forms and striking peculiarities of these organs, but also their delicate

texture. To witness the latter, the parts should be saturated with water, by immersion for a few hours, and suspended in water in a cylindrical jar, to have the aid of magnifying power: in this way the villi of the ileum, even when tanned, may be made apparent.

Although vinegar has been so long known, and so long in use, and so very much and so variously employed, yet, I believe, that for culinary purposes, it deserves to be more known and more employed, and, especially, amongst the poorer classes, with a view to economy in the preservation of food. Amongst all classes, the waste of animal and vegetable food, from not keeping, is great,—diminishing the quantity and enhancing the price. Now, by means of vinegar, it is practicable to prevent this waste,—and it is most easily effected. In the hottest weather, during the summer in the Mediterranean, when cold meat became tainted and unfit for use in twenty-four hours, or even in a shorter time during the prevalency of the damp sirocco wind, I have had slices of roast and boiled meat, of fish, poultry, &c., put into vinegar, with a proportion of gravy or any plain sauce, and they were preserved perfectly good and fit for the table for weeks. It is well known how long vegetables may be thus kept. Fruit may be preserved in the same way; and many kinds probably, would be palatable and wholesome so preserved. In Asia Minor, and in the Ionian islands, the natives, as I have witnessed, are in the habit of

keeping ripe grapes and ripe olives in vinegar,—and both are far from unpalatable; the grape retains much of its sweetness, though a little disguised by the taste of the acid; and the oil is preserved in the olive, free from rancidity, and of a very delicate flavour.

The formation of vinegar, naturally, in fruit and their juices, and in milk, seems as if it were intended, to insure their longer preservation for the use of animals as articles of food, as much so, as the changes beyond this, connected with decomposition and a new arrangement of the elements, appear destined to form manure and furnish food for plants.

XXI.

EXPERIMENTS AND OBSERVATIONS ON THE PROPORTIONAL QUANTITY OF ANIMAL AND CALCAREOUS MATTER IN THE BONES OF DIFFERENT ANIMALS.

At distant intervals, my attention has been directed to the comparative composition of bone. The results which I have obtained, I shall now bring forward. Although they are far less numerous than I could wish, and too few for full and satisfactory comparison and induction, yet, I am not without hope, that as a contribution they may conduce to these ends.

The trials which I have instituted, have been of a simple kind, with a view to determine principally the proportion of calcareous and animal matter; and this has been effected by calcination, or exposure to the action of fire in a crucible until the whole of the animal matter was consumed.

For the sake of brevity, I shall give the results in the form of Tables, and I shall include in them the earliest which I obtained so long ago as 1811, and

which Dr. Monro did me the honour of introducing into his "Elements of Anatomy." Indeed these early experiments were made at his request, and principally on specimens which he was so obliging as to furnish me with for the purpose.

Table I.

Proportion of Calcareous and Animal Matter in Human Bone, of different Races, &c.

No.	Description of Bones.	Calcar. Matter.	Animal Matter.
1	Pars petrosa of the temporal bone of an adult (Scotch?) from Dr. Monro's museum: dry: how prepared not known	66·7	33·3
2	Temporal bone of the same, under the zygomatic process	65·3	34·7
3	Occipital bone of the same	60·0	40·0
4	A part of the lower jaw of the same between the symphysis and processes	59·5	40·5
5	Portion of parietal bone of another adult (British) from the same museum, its history unknown	64·4	35·6
6	Parietal bone of another adult	62·5	37·5
7	Shaft of tibia of an adult	64·0	36·0
8	Shaft of thigh bone of an adult	62·5	37·5
9	Occipital bone of an old man	69·0	31·0
10	Part of lower jaw of an old person, between the symphysis and processes: alveoli absorbed: the bone brittle	56·6	43·4
11	Occipital bone of a young person æt. about 15, recent, previous to drying	58·0	42·0
12	Frontal bone of the same	58·9	41·1
13	Parietal bone of the same	58·8	41·2
14	Tibia of the same	53·6	46·4
15	Fibula of the same	44·0	56·0
16	Ileum of the same	45·0	55·0
17	Thigh bone of the same	47·0	53·0
18	Molar tooth of a man æt. 20, (sound)	72·4	27·6
19	Ditto ditto 23, ditto	75·0	25·0
20	Ditto ditto 30, ditto	75·3	24·7
21	Ditto ditto 31, ditto	73·1	26·9
22	Ditto ditto 32, ditto	71·0	29·0
23	Portion of parietal bone of J. Keefe, (Irish) æt. 33: died of phthisis pulmonalis, complicated with pneumathorax	68·4	31·6

No.	Description of Bone.	Calcar. Matter.	Animal Matter.
24	J. Spiers, Irish labourer, æt. 20: died of diabetes mellitus and phthisis pulmonalis*	67·5	32·5
25	M. Bayley, English labourer, æt. 21: died of phthisis pulmonalis of very rapid progress	67·2	32·8
26	J. Haliday, Irish labourer, æt. 29: died of phthisis	68·1	31·9
27	G. Cocking, English labourer, æt. 21: died of phthisis	66·6	33·4
28	T. Godfrey, English labourer, æt. 26: died of phthisis, complicated with pneumathorax of each pleura	68·4	31·6
29	H. White, English labourer, æt. 20: died of phthisis, of rapid progress . . .	67·9	32·1
30	E. Bridger, English labourer, æt. 21: died of phthisis, of very rapid progress . .	67·7	32·3
31	D. Hurley, Irish labourer, æt. 30: died of devastating disease of knee joint, complicated with tubercles and small vomicæ in lungs	67·8	32·2
32	G. Mullins, Irish, æt. 30: died of complicated disease of lungs: partial emphysema, tubercles, &c.	68·0	32·0
33	R. Allan, Scotch blacksmith, æt. 20: died of phthisis, complicated with cynanche laryngæa	66·9	33·1
34	J. M'Donough, Irish labourer, æt. 31: died of complicated organic disease without tubercles: softening of fornix: marks of old peritoneal inflammation: ascites, &c.	70·2	29·8
35	W. Macpherson, Scotch labourer, æt. 52: died of complicated disease, connected with an enlargement of heart and arch of aorta: partially ossified	68·5	31·5
36	H. Higgins, English baker, æt. 45: died of suppurative inflammation of right elbow and ankle joint	66·6	33·4
37	R. M'Cartney, Irish, æt. 28: cause of death obscure: probably syncope: it was sudden: a false aneurism, not ruptured, just below the arch of aorta	67·0	33·0
38	P. Kelly, Irish, æt. 48: died of complicated disease: softened fornix: chronic inflammation of bronchia	68·6	31·4
39	J. Masterson, Irish labourer, æt. 38: died of œdema of lungs, and thickening of small intestines after inflammation . .	68·5	31·5

* When not specified, it is to be understood, that the bone is of the same denomination as the preceding, and a similar part, and dried in the same way.

No.	Description of bone.	Calcar. Matter.	Animal Matter.
40	J. Gray, English labourer, æt. 36: died of hydrothorax and œdema of lungs: (cranium subjected to coction without lime in the water, and it was afterwards greasy) .	65·9	34·1
41	R. Stewart, Scotch, æt. 28: died of hepatized and œdematous lungs supervening on ulceration of tibiæ, enlargement of liver (it weighed 13 lbs. and contained no fatty matter)	69·4	30·6
42	C. Sidney, English, æt. 33: died from the bursting of a softened medullary tumor of the liver into cavity of abdomen . .	67·0	33·0
43	J. Shrub, English, æt. 42: died of hepatized lung, &c. complicated with a large aneurismal tumor, pressing on gullet and right bronchus	63·5	36·5
44	J. Fairweather, Scotch labourer, æt. 34: died of a ruptured aneurism of the abdominal aorta (bone subjected to coction without lime in water; not apparently greasy) . .	68·4	31·6
45	J. Woods, Irish labourer, æt. 40: died shortly after disembarkation debilitated by sea scurvy (bone submitted to coction without lime, slightly greasy)	64·4	35·6
46	W. Bickers, English weaver, æt. 44: died from the effects of sea scurvy (bone treated like the last)	64·4	35·6
47	T. Jones, æt. 49: died from the effects of sea scurvy *	66·7	33·3
48	G. Mc Carthy, Irish, æt. 31, maniacal: died of hepatization and partial œdema of the lungs	65·1	34·9
49	J. Brennan, æt. 34, imbecile: died of hepatization of lungs	66·1	33·9
50	J. Lewis, English, æt. 41, maniacal: died of tubercular phthisis	65·5	34·5

* In these three cases the fatal event was probably owing to effusion into the lungs; in each there was œdema of these organs to a considerable extent, and besides this, no lesion in any part of the body of any consequenee, in connexion with the eause of death.

It is to be regretted, that sea scurvy which for many years was almost unknown amongst our invalids returning from our distant colonies, has lately become of not unfrequent occurrence, especially amongst old soldiers from India; and is to be attributed mainly to due attention not being paid to the health and comfort of the men on these long voyages; the provisions are often of bad or indifferent quality; lime juice is seldom supplied in sufficient quantity or given in a judicious manner, and there is too often a neglect of attention to dryness and ventilation between decks.

No.	Description of Bone.	Calcar. Matter.	Animal Matter.
51	J. Green, Irish weaver, æt. 25, imbecile: coma following convulsions and partial palsy preceded death; blood effused under dura mater, softening of fornix; partial hepatization of lungs	66·9	33·1
52	J. Tomlinson, English, æt. 37: maniacal, died of partial hepatization and gangrene of lungs	68·5	31·5
53	J. Hanson, Dane, æt. 61, imbecile; died of malignant disease of orbit and liver (tumors)	64·1	35·9
54	Anne Hartly, English, æt. 51, maniacal: died of cancer of stomach (medullary tumor) .	64·7	35·3
55	J. Ball, English labourer, æt. 42, maniacal: died of complicated disease with fibro-cartilaginous tumors of neck, compressing larynx and gullet	64·9	35·1
56	Portion of frontal bone (of a very beautiful form) of a young Greek woman, of Ipsara, (it had been exposed to the air two years) .	65·2	34·8
57	Portion of parietal bone, Albanian Greek, of middle age (how prepared not known) .	69·8	30·2
58	Portion of parietal bone of a Burmese woman, middle age (how prepared not known) .	66·5	36·3
59	Canadian Indian, middle age	64·6	35·4
60	Hindoo	63·7	36.3
61	Hindoo	65·8	34.2
62	Hindoo	64·9	35.1
63	Hindoo*	66·7	33·3
64	Hottentot, middle age	65·8	34·2
65	Bojesman	62·6	37·4
66	Ashantee	68·7	31·3
67	Ashantee	68·3	31·7
68	Ashantee	68·1	31·9
69	A portion of parietal bone of Ashantee .	68·8	31·2
70	Ashantee	68·6	31·4
71	Ashantee †	69·3	30·7
72	Occipital bone of a negro (race unknown) from Dr. Monro's museum: very hard, compact and white, dry, but not dried on stove .	59·5	40·5
73	Occipital bone of another adult negro, from the same museum, less compact and hard .	58·9	41·1

* The four crania of Hindoos appeared to be of middle age; they had no marks of having suffered from exposure to the weather. They varied in size, form and thickness; the weights of similar pieces cut out with the trephine, were 23·4 grs. 38·9, 49·6, 27·6.

† The Ashantee crania were taken from a field of battle, on which a large number of these people fell, when they last invaded Sierra Leone; the exact time of their exposure is not known, probably it was less than a year.

In this First Table of results, the specimens of bones from No. 1 to 17, were miscellaneous, from Dr. Monro's museum; they belonged, it is understood, to natives of Great Britain. Some were recent and moist; others dry, but not deprived of hygrometrical moisture before being subjected to trial. They afford proof, at least, that the proportions of calcareous and of animal matter in different bones of the same body, are, as indeed might be expected *à priori*, somewhat different.

The specimens, the subjects of trial, extending from No. 18 to 71, were from the museum at Fort Pitt. Previous to incineration, they were thoroughly dried, by exposure on a stove to a temperature of about 212°, till they ceased to lose weight. This is a circumstance of some importance in comparative experiments: if neglected, the results will be of little value for the purpose of comparison, whether the bones are recent, impregnated with their peculiar fluids, or dried by exposure to the air.*

* The variable proportion of the peculiar fluids of recent bones, is, I believe, as great, if not greater, than of the solid contents: the recent parietal bones of Nos. 42 and 43, lost by drying, the former 18·49 per cent., the latter 23. The proportion of hygrometrical water is always changing in bone, as in other porous substances, according to the state of atmosphere in relation to humidity. I have found apparently dry bone lose by the expulsion of its hygrometrical water from 4 to 7 per cent. The power of absorbing moisture by different bones, probably varies with their other qualities, as is witnessed in substances generally, and especially those used as articles of dress: I may offer an experimental illustration, and I am the more disposed to do so, as the subject has not received that attention,

The specimens from No. 23 to 55 were from crania, prepared for the museum, by the process of boiling, and in the majority of instances, in water in which lime was suspended. To ascertain the effects of the process, a portion of the parietal bone No. 25, before being subjected to it, immediately after the post mortem examination, was deprived of its periosteum, and thoroughly dried and then incinerated; the residue of calcareous matter was 66·2 per cent., indicating 33·8 per cent. of animal matter,

which, practically considered, and especially in relation to health, it deserves.

On the 11th November, the substances which will presently be mentioned, were weighed in a room in which was a fire, and where the thermometer stood at 60°, and a thermometer with its bulb moistened used as an hygrometer at 56; the weather was very damp; they were then taken to an alcove in the garden, where the dry thermometer was 50°, the moist 49°; after having been left there an hour, they were weighed on the spot; and next they were dried by the fire completely till all hygrometrical water was expelled, and weighed warm. The results were the following:—

	Room.		Alcove.		Fire-side.		Lost per cent.
Fine flannel . .	37·2 grs.	—	39·5	—	32·9	—	17·0
Brown merino .	23·6	—	24·	—	20·5	—	14·6
Fine silk sarsenet	13·9	—	14·1	—	13·	—	7·8
Fine muslin . .	13·5	—	14·1	—	12·5	—	11·3
White kid glove	18·6	—	20·2	—	16·4	—	18·8
New linen . . .	68·45	—	71·6	—	64·1	—	10·4
Old linen . . .	53·	—	54·4	—	49·	—	9·9

The same substances were placed next the skin over the chest, where the thermometer was 89°, covered with a shirt, flannel waistcoat, &c. after several hours they were found to weigh as follows:—

Flannel 35·6, merino 22·25, silk 13·5, muslin 13·05, kid glove 17·5, new linen 66·2, old linen 50·9.

instead of 32·8 after the boiling, thus shewing a removal of 1 per cent. of animal matter by the boiling process.

How the remaining specimens, excepting No. 56, were prepared is not known. They were sent from foreign stations, and were probably previously macerated in water or left exposed to the air, like No. 56, till the soft parts had been destroyed by the action of the elements.

The crania extending from No. 23 to 55, it will be seen, were from four descriptions of patients:—1. those who had died of tubercular phthisis; 2. those who had died of other diseases not connected with tubercle; 3. those who had died of sea scurvy; and 4. insane persons. The majority of them were part of a series preserved during twelve months, taken from every fatal case which occurred, and put aside for the purpose of comparison.

The average proportion of calcareous matter afforded in these four different classes, was the following per cent:—

Phthisis	67·7
Other diseases unconnected with tubercles	67·6
Sea-scurvy	65·1
Insanity	67·5

With the exception of the instances of sea-scurvy, these differences are so small, that perhaps it may appear questionable, that they are owing to disease: in sea-scurvy, it has been asserted, that the bones have been affected, and that old fractures have

become disunited. Three cases are hardly sufficient to inspire confidence in the conclusion. In each of the three, however, the proportion of calcareous matter being below the mean of all the others, is very favourable to the idea that in this disease there is actually an absorption of the earthy matter of the bones.

As the manner in which the crania of the foreign races submitted to trial were prepared is not known, no satisfactory inferences can be drawn relative to the proportions of the calcareous and animal matter in them. The results are so very different, especially in the African instances, that if of no other use, they are well adapted to prevent hasty generalization.

Table II.

Proportion of Calcareous and Animal Matter in Bone in Early and in Advanced Life.

No.	Description of Bone.	Calcar. Matter.	Animal Matter.
1	Portion of margin of parietal bone of a fœtus of six months	50·0	50·0
2	Portion of margin of parietal bone nine months old	62·1	37·9
3	Portion of the same towards the centre .	63·4	36·6
4	Frontal bone of a child, the bregma still remaining, from Dr. Monro's museum .	54·5	45·5
5	Parietal bone of the same	54·0	46·0
6	Lower jaw of a child from Dr. Monro's museum	57·2	42·8
7	Milk teeth of a girl, shed at usual time, the roots absorbed: thoroughly dried, very brittle and fragile (three front ones weighed only 5·94 grs.)	79·5	20·5

No.	Description of Bone.	Calcar. Matter.	Animal Matter.
8	Hollow summits of teeth of inferior jaw of a fœtus, between five and six months, several years preserved in spirit of wine, (they weighed only ·4 gr.) enamel not deposited	75·5	25·0
9	Under-jaw of the same, connected by ligament at symphysis, deprived of periosteum, nerve, &c. thoroughly dried, weighed only 8·5 grs.	56·0	44·0
10	Portion of parietal bone of a Maltese woman, æt. 98: very thick and heavy, abounding in stearine	47·6	52·4
11	Portion of parietal bone of a Maltese woman, æt. 83: unusually thick: it contained very little fatty matter	66·0	34·0
12	The occipital bone of an old man, from Dr. Monro's museum	69·0	31·0
13	A part of the lower jaw of an old person, between the symphysis and processes: the alveoli were absorbed, the bone brittle .	56·6	43·4
14	Portion of radius from an old woman whose bones had been frequently broken, and were supposed to be peculiarly brittle, from Mr. Liston's museum	66·0	34·0
15	Molar tooth of a man, æt. 61: weighed 31·9 grs.	73·6	26·4

The bones noticed in this Second Table, with the exception of those from Dr. Monro's museum, were thoroughly dried previous to incineration. Both on account of the small number of specimens tried, and it not being known how they were all prepared, I shall not attempt to deduce any conclusions from the results: I have thought it right to give them, with the hope that they may excite doubt, and lead to further inquiry. There being no deficiency of calcareous matter in the bone No. 10, belonging to an old person who had repeatedly suffered from the accident of fracture of bone,—would seem to indicate (supposing the tendency to fracture to be in the bone) that weakness and brittleness may arise

from other circumstances than deficiency of phosphate of lime. This is a point deserving special inquiry. Probably it will be found that in regard to strength and power of resistance, more depends on the arrangement of the constituent particles, and the proportion of fluid matter interposed, than on the quantity of solid contents, especially the calcareous portion.

Table III.

Proportion of Calcareous and Animal Matter in Bone of different Animals.

No.	Description of Bone.	Calcar. Matter.	Animal Matter.
1	Portion of the tympanum of a whale, the species not determined: of specific gravity 224, and after having been subjected to the air-pump 246	81·2	18·8
2	Pars petrosa of the temporal bone of another whale, species not known, from Dr. Monro's museum	72·6	27·4
3	Pars petrosa of an elephant, from the same museum	70·0	30·0
4	Under-jaw of another elephant . .	70·8	29·2
5	Under-jaw of hippopotamus . . .	67·3	32.7
6	Under-jaw of dromedary	68·4	31·6
7	Metatarsal bone of a pure-bred horse, "Lorenzo," six years old, (just taken from the maceratering tub) of specific gravity 1854, and after having been subjected to the air-pump 2033: it lost by thorough drying 9·5 per cent.	65·77	34·23
8	Metacarpal bone of a troop horse rather lower bred than usual, (prepared partly by macerating, partly by boiling) of specific gravity 2010, before action of air-pump, and 2077 after: it lost by thorough drying 5·45	65·78	34·22
9	Shaft of humerus of a blood-horse (the compact part) before being subjected to air-pump, of specific gravity 2045: after, 2092: it lost by thorough drying 7 per cent. .	69·44	30·56

No.	Description of Bone.	Calcar. Matter.	Animal Matter.
10	Similar part of shaft of humerus of a dray-horse, before being subjected to the air-pump, of specific gravity 2000: after, 2126: it lost by thorough drying 6 per cent. .	70·8	29·2
11	Parietal bone of Bombay buffalo, close to the horn	60·3	39·7
12	Horn of the same, near its base . .	3·02	96·98
13	Parietal bone of Persian goat . . .	66·4	33·6
14	Ditto of Bengal antelope (goral) . .	64·7	35·3
15	Ditto of damalis risea, Bengal . .	66·5	33·5
16	Ditto of cervus capriolus . . .	61·3	38·7
17	Ditto of sheep of Southern Africa . .	57·1	42·9
18	Extremity of process of parietal bone of the same, supporting the horn . . .	57·6	42·4
19	Portion of horn of the same . . .	2·7	97·3
20	Parietal bone of wild hog (Bengal) . .	67·3	32·7
21	Superior portion of tusk of the same . .	71·9	28·1
22	External table of parietal bone of another wild hog	63·7	36·3
23	Parietal bone of tiger	65·4	34·6
24	Cellular structure of inner table of frontal bone of the same	65·6	34·4
25	Incisor tooth of the same	69·5	30·5
26	Molar tooth of the same	72·1	27·9
27	Lower jaw of common cat . . .	71·6	28·5
28	Parietal bone of dog	68·2	31·8
29	Ditto jackall	68·4	31·6
30	Ditto wolf	70·5	29·5
31	Ditto hare	61·7	38·3
32	Shaft of tibia of tame rabbit . . .	65·8	34·2
33	Ditto of femur of a young kangaroo .	60·0	40·0
34	Ditto of mustela vulgaris, rather oily .	66·6	33·4
35	Ditto putorius vulgaris	61·5	38·5
36	Parietal bone of full-grown seal (species unknown)	63·0	37·0
37	Ditto of another seal . . .	68·4	31·6
38	Ditto of another seal . . .	70·1	29·9
39	Shaft of femur of vespertilio vampyrus, Bengal	60·1	39·9
40	Ditto of vespertilio murinus, oily . .	57·0	43·0
41	Ditto of vespertilio noctula, oily . .	55·8	45·0
42	Ditto of radius of petaurus macrourus, oily and semi-transparent	62·8	37·2
43	Ditto of tibia and fibula of bathyergus capensis	63·0	37·0
44	Ditto of pteromys petaurista, oily and semi-transparent	62·0	38·0
45	Ditto of tibia of a common fowl (a hen) .	68·6	31·4
46	Superior portion of sternum of the same .	55. 5	44·5

No.	Description of Bone.	Calcar. Matter.	Animal Matter.
47	Ridge of sternum of wild swan of a delicate cellular structure	57·0	43·0
48	Portion of sternum of ostrich, including the delicate cellular structure connecting the outer and inner tables	56·6	43·4
49	Parietal bone of a large species of falcon .	63·9	36·1
50	Parietal and frontal bone of pelican . .	63·3	36·7
51	Ditto of adjutant bird (ciconia argala) elegantly cellular	67·5	32·5
52	Ditto of bucceros caratus	60·6	39·4
53	Portion of lower jaw of diomedea fuliginosa .	57·2	42·8
54	Portion of spine of testudo græca . .	57·0	43·0
55	Sternal portion of the shell of the same .	59·0	41·0
56	Portion of lower jaw of turtle of large size (Ascension—chelonia midas) . . .	67·5	32·5
57	Ditto of gaviala tenuirostris . . .	69·6	30·4
58	Ditto of trigonocephalus lanceolatus (St. Lucia)	70·1	29·9
59	Poison fang of the same *	62·1	37·9
60	Portion of upper jaw of the same . .	55·9	45·1
61	Shaft of femur of common frog . . .	68·6	61·4
62	Ditto common toad; it was more oily than the preceding	58·1	41·9
63	Vertebræ of cod-fish, (they lost by thorough drying 32·7 per cent.)	58·7	41·3
64	Portion of bony integument of ostracion quadricornis (it was dry, by thorough drying lost 5·7 per cent. hygrometric water) .	40·4	59·6
65	Teeth of dog-fish (mustela lævis) . .	50·0	50·0
	Its skin yielded	24·0	76·0

All the bones noticed in this Third Table, excepting Nos. 2 and 3 were thoroughly dried previous to incineration; and the majority of them belonged to the museum at Fort Pitt.

The difference in the proportions of the animal and calcareous matter constituting them is less strongly marked than might have been expected; and, as it appears to me, not sufficiently great and

* The canal in the fang of this snake was found to be in part cellular, its middle portion; after calcination, the part was very easily examined.

constant in animals of the same natural families, to admit of any satisfactory conclusion being drawn on the subject. Nor is it probable that any such conclusion can be drawn, until a series of comparative experiments shall have been instituted, conducted with minute attention to accuracy, and under circumstances, especially as regards the method of preparation, precisely similar.

It has been asserted that there is as marked difference in the proportions of the constituent parts of bone of the blood-horse and the cart or dray-horse, as there is in their appearance and some of their physical qualities ; that the former, though stronger and more compact than that of the latter, is of lower specific gravity, and contains a less proportional quantity of calcareous matter. To endeavour to satisfy myself on these points, I have been at some trouble to procure for examination, specimens of bone of the varieties of horse in question ; for those of the thorough-bred horse "Lorenzo" and of a common life-guards' troop-horse, I am indebted to Mr. Gulliver. The results they have afforded are not in confirmation of the opinion referred to ; they seem rather to favour a conjecture, already proposed, that in relation to strength more depends on the arrangement of the constituent particles than on the proportion of calcareous matter ; and indeed many other of the results of the comparative trials contained in this table, point the same way.

Table IV.

Proportion of Calcareous and Animal Matter in Exhumed and Fossil Bones.

No.	Description of Bone.	Calcar. Matter.	Animal Matter.
1	Portion of parietal bone of a human cranium, from an ancient tomb in Cerigo . .	73·8	26·2
2	Portion of zygomatic process of the temporal bone of an ancient Egyptian cranium from a tomb in Thebes; in form like the finest Grecian	76·1	23·9
3	Frontal Roman bone, found at Pompei .	64·5	35·5
4	Bone found at Borrowstownness, included in sandstone, apparently a human tibia .	83·2	16·8
5	Bones from the banks of the Ohio, reddened by oxide of iron and penetrated by extraneous earthy matter, said to have been found at the depth of twelve feet . . .	69·0	31·0
6	A tooth of the mammoth	69·5	30·5
7	The enamel of the same tooth . . .	82·6	17·4
8	Portion of os humeri of a horse, found in clay near Upnor Castle in Kent, twenty-four feet below the surface . . .	77·0	25·0
9	Portion of a cylindrical bone of an elephant from a cavern in Corfu	95·0	5·0

For the few examples in this Fourth Table, I have been indebted to Dr. Monro, with the exception of Nos. 1, 2, 8, and 9. No. 1, I received through the kindness of Major Macphail, who I understood found it in an ancient Greek tomb, in Cerigo, cut out of the porous calcareous free-stone of that island; and No. 2 was given me by the late Mr. Henry Stodart, who, on his return from Upper Egypt, assured me he had taken it himself from a tomb in ancient Thebes. Both skulls are brittle, the former is probably more than 2000 years old; and the latter not less than 3000.

The considerable proportion of animal matter which these bones contain, all but the last, is remarkably contrasted with the total or almost total want of it, in bones belonging to the most recent rock-formations. In the bone breccia of the Mediterranean,—so widely scattered,—I have been able to detect a just perceptible trace only of animal matter: and in the teeth of the squali, which occur in the tertiary formations of Malta and Gozo, I have not been able to detect even a trace of it. In an enormous tooth* of one of these fishes now in my possession,—I carefully sought for animal matter, but in vain. They and the fossil bones generally which have not been exposed to the air, owe their strength and hardness to a kind of cement of carbonate of lime, which they all acquire. Judging analogically from the partial effect of a known period of time, what an idea of vast antiquity is conveyed by the circumstance of the total destruction of the animal matter of bones!—Perhaps, the instance of the Borrowstownness bone, mentioned, in which the animal matter was only partially decomposed, may be considered as in opposition to the preceding remark. But it hardly can fairly be so considered, taking into account that it is probably human,—as its form indicates, and that the sandstone is probably of very recent formation, and may be even still in progress.

* It is 6¼ inches long; 1 inch thick where of greatest thickness, and 4¾ wide where of greatest width.

Table V.

Proportion of Calcareous and Animal Matter in Diseased Bones.

No.	Description of Bone.	Calcar. Matter.	Animal Matter.
1	The body of a large thigh-bone of an adult affected with hyperostosis, very thick and hard, but not very compact . . .	76·9	23·1
2	The bone of a person supposed to have died of lues venerea, thick, light and spongy in a slight degree*	70·7	29·3
3	Cancelli of a curved tibia	74·5	25·5
4	The compact body of the same bone .	63·0	37·0
5	A portion of rickety parietal bone, about an inch thick	72·9	27·1
6	Another portion of the same bone . .	69·5	30·5
7	The body of a rickety thigh bone, very thick	62·2	37·8
8	An exostosis	63·8	36·2
9	A scrofulous exostosis	63·0	37·0
10	A spinal process of a lumbar vertebra of the curved spine of a rickety person . .	59·3	40·7
11	A rib of the same person	59·2	40·8
12	The tibia of a rickety child, soft and spongy	26·0	74·0
13	A deformed female pelvis: soft and porous, and not unlike horn in appearance .	24·2	75·8
14	Portion of detached necrosed bone . .	59·2	40·8
15	Callus of fractured femur of an adult, supposed to be very old: in appearance very dense,—denser than the shaft . .	52·4	47·6

* Death from lues venerea seems to be very problematical; and disease of bones as an effect of syphilis, seems also to be problematical. After an experience of nearly 25 years in the army, I do not recollect having witnessed a fatal event consequent on lues venerea fairly attributable to it alone. When such an event has occurred more or less remotely following syphilis,—tubercles or other organic disease have been developed, and have been the principal cause of death. And I have never met with any instance in which in secondary syphilis the bones had been diseased, unless mercury had been used. In offering these remarks, it is not my intention to maintain that secondary syphilis is never complicated with disease of bones: it may be so, and any other disease may be so complicated, accidentally, not essentially,—bone being liable to morbid action from a variety of causes, and which may take effect during the progress of other diseases.

No.	Description of Bone.	Calcar. Matter.	Animal Matter.
16	Shaft of the same bone, about two inches distant from the callus, (Fort Pitt Museum, Loc. Div. 4, No. 81.)	59·8	40·2
17	Callus of fractured femur of a person æt. 43 .	50·4	49·6
18	Shaft of the same close to the callus, (Fort Pitt Museum, Loc. Div. 4, No. 65.) .	55·9	44·2
19	Callus of another fractured long bone from the same museum	61·2	38·8
20	Shaft of the same bone, which was more easily incinerated than the callus . . .	61·4	38·6
21	Callus of a fractured femur of a soldier, who died four years after the accident: very compact	62·8	37·2
22	Shaft of the same, about an inch from callus, (F. P. M. Loc. Div. 4. No. 19.) . .	61·2	38·8
23	Callus of a very old fractured femur, (its exact age not known) very compact . .	58·3	41·7
24	Shaft of same, about 1 inch from callus, (F. P. M. Loc. Div. 4, No. 20.) . . .	59·4	40·6
25	Fractured end of bone of leg of a rabbit, difficult to saw, 29 days after injury (from Mr. Gulliver)	65·4	34·6
26	Callus uniting the fractured ends of the same bone	54·6	45·4
27	Healthy portion of the shaft of the same .	65·8	34·2
28	Exfoliation from human clavicle, presented to the museum by Dr. Williams . .	58·0	42·0
29	Necrosed end of femur after amputation of limb	65·4	34·6
30	Portion of shaft of the same, near the end .	63·6	36·4
31	Portion of the same high up . . .	60·0	40·0
32	Necrosed shaft of femur	60·6	39·4
33	New bone enveloping the necrosed shaft, (F. P. M. Loc. Div. 4, No. 22.) . . .	57·1	42·9
34	Portion of necrosed tibia, near the head of the bone, its outer compact part . .	63·4	36·6
35	Portion of new bone enveloping the preceding, (F. P. M. Loc. Div. 1, No. 95.) .	43·6	56·4
36	Necrosed tibia of a boy æt. about 16 . .	60·0	40·0
37	New bone belonging to the same limb,* (F. P. M. Loc. Div. 1, No. 169.) . .	53·6	46·4
38	Porcelain-like deposit, (hard, compact, polished) on head of humerus, deprived of its cartilage, (F. P. M. Loc. Div. 2, No. 49.) .	54·2	45·8

* In this instance, as is generally, if not invariably the case, the new bone deposited to supply the place of the dead bone, showed no regularity of structure; after incineration, it was crushed to powder by moderate pressure.

No.	Description of bone.	Calcar. Matter.	Animal Matter.
39	Portion of new bony matter deposited round the margin of the articular surface of the preceding	48·8	51·2
40	Portion of healthy shaft of the same bone: from a woman, who for many years had been subject to the gout . . .	58·8	41·2
41	Portion of tibia from G. Snellings, æt. 17: whose leg was amputated on account of caries with softening of ankle-joint, (F. P. M. Loc. Div. 2, No. 49.) . . .	38·2	61·8
42	Upper part of os humeri exhibiting osteo-sarcoma, so soft, as to be easily cut (F. P. M. Loc. Div. 2, No. 29.)	39·6	60·4
43	Tibia thickened and softened of an adult who had taken large quantities of mercury, (F. P. M. Loc. Div. 1, No. 33.) . . .	60·0	40·0
44	Portion of os calcis from a limb amputated on account of disease resembling elephas: by drying, it lost 37·5 per cent.: it was soft, and abounded so much in oil, that it was semitransparent*	15·0	85·0
45	Molar tooth of a soldier said to have been loose four years	68·5	31·5

* The limb was amputated by Mr. Shelly of Epsom, and by him presented to the Museum at Fort Pitt, where it is now deposited. It would appear from the information afforded by this gentleman,—that his patient Thomas Grainger, an invalided artillery man, received a slight contusion on the dorsum of the left foot at the Cape of Good Hope in 1806; that after apparent recovery, ulcers of a severe and intractable kind formed about the left ankle, for which he was sent home and discharged the service; that after deriving benefit from change of climate,—he had a febrile attack in 1815, followed, whilst he was a patient in St. Thomas's Hospital, by swelling and ulceration of the foot; and with which he was discharged, the disease being considered incurable. In 1834, when Mr. Shelly first saw him, the leg had acquired an enormous size, and was discharging a very offensive sanies. He then objected to an operation. Ulceration continued and increased, the bones of the toes, it is stated, becoming carious and gradually disappearing. Life being in danger from the state of the limb, and from the hæmorrhage which occasionally took place from the ulcerated parts, in the last week of August, 1838, he submitted to the operation of amputation,

In comparing the results in this last Table, it appears, how very variable is the proportion of calcareous matter in diseased bone; and what very little agreement there is between the quality of hardness and of softness of bone, and the proportions of calcareous and animal matter; further confirming the conjecture that more seems to depend, in relation to these qualities, on arrangement of the ingredients than on their respective proportions. Undue softness, it would appear, may co-exist either with animal matter in excess, which is the most common occurrence; or in about the average quantity, or even in deficiency, which is far more rare. Perhaps more extended research may lead to clearer general views of the connexion of the pro-

which was performed above the knee. The wound rapidly healed, excepting close to the ligatures; on the 21st October, nearly nine weeks after the removal of the limb, one ligature was still attached. The most remarkable appearances, which presented on examining the limb on its arrival at Fort Pitt, were the softened spongy state of the bones abounding in oil, (the tarsal and what remained of the metatarsal bones, actually floated when thrown into water) a conversion of the muscles more or less into fatty matter; a condensed state of cellular tissue, with much accumulation of fat, and a hypertrophied state of the cutis and cuticle. In Ceylon many years ago, I had an opportunity of making a post-mortem examination of a native who died in the Leper-Hospital, of elephas, complicated with elephantiasis, (that is, the enlarged leg with the tubercular disease of the skin) and the appearances of the affected limb were closely similar to the preceding. An account of the dissection is to be found in the last chapter of my work on the Interior of Ceylon. In this instance, the disease extended to the thigh: a large quantity of oil was found in the capsule of the knee-joint in the place of synovia. The bones were not examined.

portions of calcareous matter with certain diseased states and physical conditions according to precise rules. But on this head there is little encouragement to be sanguine at present: the limited experience which we possess, does not point to any such rules. The subject is one of difficulty, and of little promise, and therefore not likely to be prosecuted with the diligence and care requisite to discover order; and which assuredly can only be effected by the simultaneous study of the pathological state of the part, and its chemical composition, following it in its different stages.

In application to particular questions, the results contained in this Table, I would hope, even limited as they are, may be of some use.

XXII.

OBSERVATIONS ON THE CONTENTS OF THE URINARY BLADDER AFTER DEATH.

I HAVE been induced to pay some attention to this subject, from finding in an instance of diabetes complicated with tubercular phthsis, the urinary secretion, shortly before death, restored nearly to its healthy condition. The outline of the case was the following :—

J. Spiers, æt. 20, three weeks after enlistment was found to labour under diabetes. After treatment for nearly six months in the hospital of the regiment to which he belonged, he was sent to the general hospital at Fort Pitt, preparatory to being discharged the service. On admission, the symptoms of diabetes were well marked and characteristic ; those of phthisis, less so. The urinary secretion in the twenty-four hours amounted to six quarts, it was of sp. grav. 1040, and abounded in saccharine matter. He lived two months longer. Towards the termination of the disease the pectoral symptoms increased, the diabetic diminished ; two

days before his death, the urine was of sp. grav. 1017, and a few hours before of 1022, and was found to abound in urea, and to contain very little saccharine matter.

On inspection of the body, cavities and tubercles were detected in both lungs; and no other material organic lesion was discoverable.

The urinary bladder contained about 1½ pint of urine, of sp. grav. 1014, clear and of a bright brown hue. It was found on chemical examination to contain a large proportion of urea, without any appreciable quantity of saccharine matter.

Exclusive of this case, I have notes of twenty-seven others collected at Fort Pitt, in which, the inquiry, post-mortem, relative to the urine was prosecuted. I shall give the results in a tabular form; stating the quantity of fluid found in the bladder; its appearance under the microscope when any wise peculiar; its specific gravity, ascertained by a delicate balance; its acidity (when possessed of that quality) by litmus paper; whether serous or albeminous, in a marked degree, or not, by the test of nitric acid; and the presence in it of urea, by the effect of the same acid on it, when concentrated by evaporation over boiling water, to the consistence of syrup. No selection of cases was made for the examination; almost every fatal case that occurred during a period of nearly six months was taken advantage of for the purpose; and I shall introduce the results obtained from each, in the order of time,

rather than classed according to the nature of the fatal disease,—this being most in accordance with the experimental plan.

Table

Of Observations on the Urinary Bladder and Urine after death.

No.	Age.	Principal organic lesions discovered by autopsia.	Hours after death.	Observations on the urine.
1	51	Cancer of stomach (medullary tumour softening, with ulceration)	17	About 8 oz. found in the bladder: ale-coloured: of sp. gr. 1015: not serous: abounding in urea
2	33	Tubercles and cavities in lungs, with pneumathorax from perforation of pleura of right lung	11	About 1 dram of urine in bladder: of natural colour and appearance
3	58	Partial gangrene of left lung: 12 pints of serum in cavity of abdomen: peritonæum thickened and granular	6	About 4 oz.: of natural brownish hue, with slight mucous sediment: sp. gr. 1013: not serous: abounding in urea
4	21	Tubercles and cavities in lungs, &c.	9	Bladder quite empty
5		Ditto ditto	31	1 dram: turbid: serous
6	25	Ditto ditto	16	6 oz.: porter-coloured: sp. gr. 1019: abounding in urea
7	42	Partial hepatization of lungs: a large aneurism of thoracic aorta, not ruptured	12	A few drops only: too little for trial
8	28	Tubercles and cavities in lungs: a medullary tumor in lower portion of spinal canal. Ureters and urinary bladder bloodshot, &c.	23	½ oz.: turbid, purulent: of sp. gr. 1027: under microscope pus-globules seen in it, with a few blood-corpuscles: and particles of a smaller size: no urea detected
9	40	Partial œdema of lungs: gangrenous fistula in ano	22	3 oz.: ale-coloured: sp. gr. 1011: slightly serous: abounding in urea
10	31	Tubercles in lungs with cavities in left lung	6	1 oz.: ale-coloured: sp. gr. 1014: not serous: urea not deficient

No.	Age.	Principal organic lesions discovered by Autopsia.	Hours after Death.	Observations on the Urine.
11	25	Hepatization and œdema of lungs	21	4 oz.: porter-coloured: of offensive cadaverous odour: contained pus-like globules, and many spermatic animalcules: sp. gr. 1020: not serous: abounding in urea
12	22	Ecchymosis with bloody serum in lungs, containing pus-like globules; rima glottidis, slightly œdematous: valves of heart and large blood vessels strongly stained by colouring matter of blood: pus-like globules in blood, and in softened subst. of spleen*	12	2 oz.: ale-coloured: odour very offensive: sp. gr. 1317: abounded in urea
13	29	Cavities in left lung: tubercles in both	28	3 oz.: porter-coloured: odour offensive, cadaverous: acid: sp. gr. 1019: not serous: a few pus-like globules in it, and many much smaller particles
14	44	Hepatization and œdema, with partial gangrene of lungs		3 oz.: porter-coloured: acid: slightly serous: sp. gr. 1023: one spermatic animalcule detected: a few pus-like globules, and fine filaments: abounding in urea
15	39	Cavities and tubercles in lungs	28	1 oz.: high-coloured: odour offensive: sp. gr. 1022: slightly serous: not deficient in urea
16	33	Ditto ditto	20	No urine in bladder
17	33	Caries with partial necrosis of frontal and parietal bones: softening of fornix: tubercles and cavities in lungs	17	4 oz.: ale-coloured: sp. gr. 1006

* This case of confluent variola which proved fatal on the 14th day, on post-mortem examination afforded a striking example of red discolouration or staining of lining membrane of the heart, and great blood vessels, in connexion with red serum and incipient putrefaction, the blood mixed with lime emitted an unusually strong ammoniacal odour, and the blood particles were puckered and diminished in size. At page 194. Vol. II, will be found some observations

No.	Age.	Principal organic lesions discovered by Autopsia.	Hours after Death.	Observations on the Urine.
18	21	Cavities and tubercles in lungs	21	2 oz.: porter-coloured: not serous: sp. gr. 1017
19	35	Lungs partly œdematous, partly hepatized, and containing puriloid fluid. Puriloid matter and fibrinous concretions, obstructing iliac veins, connected with sea scurvy	28	A few drops only
20	47	Ulcers in rectum and lower portion of colon: a perforation into cavity of abdomen, with gangrene and marks of peritoneal inflammation	15	3 oz.: ale-coloured: acid: slightly serous: sp. gr. 1015
21	18	70 oz. of serum of sp. gr. 1019 in left pleura: 27 in right of the same sp. gr. 3½ oz. in pericardium of sp. gr. 1014: tubercles in both lungs and cavities in the left	37	Slightly serous: acid: sp. gr. 1016; abounded in urea: contained pus-like globules and many very minute animalcules, —thread-like,—their length equal to about the diameter of a blood-disc, some with a vibratory, others an undulatory motion
22	30	Tubercles and cavities in lungs	33	4 oz.: straw-coloured: slightly turbid: not serous: contained spermatozoa and some pus-like globules: sp. gr. 1013: abounded in urea
23	38	Lymph extensively effused over pericranium (erysipelas): two small cavities and a few clustered tubercles in lungs (latent phthisis)	6	1½ oz.: dark ale-coloured: acid: serous: sp. gr. 1021: very little urea: many globular filamentous particles, probably of mucus, and deriving their form from mucous follicles
24	31	Tubercles and cavities in lungs	25	1 oz.: high-coloured: acid: not serous; sp. gr. 1019: not deficient in urea: contained many minute particles: one spermatic animalcule was seen, and a globular animalcule in motion, smaller than a blood corpuscle

on the dependence of the staining of the heart and great vessels, on the cause just referred, viz. putrefaction and the serum of the blood holding in solution colouring matter of the blood.

No.	Age.	Principal organic lesions discovered by autopsia.	Hours after death.	Observations on the urine.
25	39	Tubercles and cavities in lungs. Large intestines several ulcerated : end of appendicula vermiformis perforated by ulceration, adhering to colon, and gangrenous where adhering . . .	13	1½ oz.: light ale-coloured : slightly turbid : acid : sp. gr. 1014 : slightly serous : contained many spermatozoa, and very minute particles : not deficient in urea
26	38	Ulceration of pharynx and larynx, with œdema of margin of epiglottis and glottis, and partially of the lungs, with destruction of a considerable portion of the palate, including the greater part of the bony plates .	34	4 oz.: pale ale-coloured : a mucous sediment on standing : sp. gr. 1027 : acid : slightly serous : contained some urea
27	33	Tubercles and cavities in lungs	29	1 oz. : ale-coloured : sp. gr. 1020 : acid : slightly serous : contained urea
28	35	Fracture of cranium, with laceration of middle meningeal artery and extravasation of a large quantity of blood within the cranium . . .	27	1 drachm : a sediment of gelatinous mucus, consisting (as seen under the microscope) of particles, globular and filamentous, like those noticed in case 23. The little supernatant fluid was acid, and resembled some urine, drawn off by catheter, ten hours before death, of sp. gr. 1010 : and abounding in urea

Do not these observations seem to indicate that the kidneys, in the majority of cases perform their function, even in the last hours of life ; and that the secreted fluid of the moribund, differs less from healthy urine, than in many instances of diseases, not threatening life ? The first case, introductory to the others,—that of diabetes complicated with phthisis, is a striking example in point. Admitting the inference to be correct, it is necessarily matter

of conjecture how the effect takes place. Other organs exhibit phenomena somewhat similar. In phthisis, shortly before death, it is not uncommon to witness a cessation of distressing pectoral symptoms, and a return of comparatively easy respiration. In insanity, complicated with organic disease of either the thoracic or abdominal viscera, and especially with tubercular disease, it generally happens that the destructive malady runs its course, without apparently deranging the functions of the organs in which it is seated. In all organic diseases, and most distinctly marked in instances of chronic disease of the heart, there are marked periods of rest, as well as of exacerbation. And during the fits of aggravation, certain medicines, as narcotics, timely administered, often have the effect of affording great and sudden relief, seemingly allaying the deranged functions and restoring the organ to its healthy state of action. Lastly, it may be remarked, that, the functions of most organs, even when free from organic disease, are liable to irregularities; as the sudden blush, the quickening of the pulse, the opening of the pores and bursting forth of sweat, the sudden change of the urinary secretion, becoming at once excessive in quantity and extremely dilute,—all connected with mental emotion. Now as in these instances, the modifying effect, appears to depend on the nervous system, exerting some peculiar influence; it seems not improbable, that in the last stage of life, a similar influence may

be exerted on the kidneys, and a kind of healthy action temporarily restored. But, this is speculation,—and as such, of little weight. Practically considered, perhaps a useful caution may be deduced, from the comparatively healthy state of the urine in death; a caution of not attaching too much importance to the condition of the secretion during life, and in instances of disease, especially on casual observation and trial. The natural inference seems to be, if in dying the urine can resume its healthy qualities, or an approximation to them, it is likely to do the same occasionally, under peculiar influences, during the prevalency of active disease. Examples of this kind are probably familiar to every experienced practitioner, who has directed his attention to the subject. And, the propriety of using caution in this matter, seems to be enforced by what is now and then witnessed in the condition of the secretion, in persons apparently in perfect health,—a condition the reverse of that alluded to in death, viz. a change from the healthy to a morbid state,—as a sudden and unexpected deposit of a large quantity of brick-red or rose-coloured sediment, to the no little alarm of the individuals, till they find (as often happens) that such a deposit is of as little consequence as the appearance of a pimple on the forehead, or of a papular eruption on the lip in sound or apparently sound health.

Reverting to the particular observations, it appears, that in two instances,—cases No. 21 and 24,

minute living animalcules were seen in the urine under microscopic examination. I believe they came from the bladder; but it is right to state, I am not certain that they did, as the vials in which the fluid was received, had previously held pump-water. In using the microscope it is impossible to proceed with too much caution, or to be too sceptical, especially when the results are in any wise extraordinary.

Further, reverting to them, it appears, that in several instances, viz. in cases Nos. 11, 14, 22, 24, and 25, spermatic animalcules were detected in the urine. Of the nature of these animalcules, there could be no question, their forms were so distinct. Nor can there be, I apprehend, any doubt entertained that they had been collected previously in the vesiculæ seminales. Probably in the convulsion of death, which is often accompanied with the discharge of a portion of the contents of these organs, some of the fluid may have been irregularly propelled into the urinary bladder.

XXIII.

SOME DIRECTIONS FOR MAKING AND KEEPING ANATOMICAL PREPARATIONS IN HOT CLIMATES.

It is too generally supposed that the making and keeping of anatomical preparations in hot climates is almost impossible, or attended with so much difficulty as to be practically impossible, with the ordinary means within the reach of medical men.

This is a mistaken notion, and it is for the interest of science that it should be refuted.* The

* My attention was directed to the above subject in 1825, when the present director-general of the army medical department, was calling on medical officers on foreign stations to contribute to the museum at Fort Pitt, then in its infancy, and which now, owing in great measure to the assistance so afforded, is become one of the most valuable collections of pathological anatomy in this or in any other country. Most of the observations contained in this paper were written at that time, and privately circulated with the view to remove the impression alluded to in the text; and were afterwards published in the 8th vol. of the Edinburgh Medical and Surgical Journal. I am induced again to bring them forward, with such alterations as extended experience has suggested, with the hope that they may be still useful in the important cause of pathological research, and of that museum which is the permanent record of such research amongst the medical officers of the army, by whom it has been formed.

changes which animal matter undergoes at a temperature between 80 and 90° Fahrenheit (the average maximum of the highest temperature, in the hottest seasons, even in intertropical climates), do not differ in *kind* from those which occur at a temperature between 45° and 55°, and *à fortiori* between 55° and 70°, which may be considered about the average in-door temperature of the winter and summer seasons in Great Britain. The difference then in the changes is chiefly in *degree:* in a hot climate they take place more rapidly, than in a temperate one, twice or thrice as rapidly, according to the elevation of temperature. This should always be kept in mind as a maxim, and principle; and to ensure success in making anatomical preparations, the rapidity of change of animal matter must be met with proportional quickness and energy of the conservative processes of art opposed to the destructive ones of nature. With the same view, and against the same tendency to change and decompose, besides quickness, great neatness, and cleanliness are requisite.

Every dissection should be conducted in a regular and scientific manner, according to a certain method, and with definite objects in view. The principal objects of all dissection are three: the detection of the effects of the disease, and the cause of death; the removal of diseased parts for preservation; and the acquisition of general anatomical knowledge. Neatness, and cleanliness, and method, conduce equally to these objects. Attending to them, ob-

scurity, confusion, and error are avoided; the pursuit loses as much as possible its disgusting aspect; it gives information of a satisfactory kind; excites interest powerfully; and zealously pursued becomes almost fascinating. Farther, when the dissection is conducted on these principles, it is the source of much valuable instruction. It makes the hand dexterous for surgical operations; it produces caution in deciding on *post mortem* appearances, which are so often deceptive; and habituates the eye to the nice discrimination of what is sound in structure, and what is diseased.

When any morbid appearance presents itself, the part displaying it should be carefully examined before it is removed; its situation should be noticed, and its connections traced. If it is considered worthy of being preserved, with that intention, it should be dissected out so as to appear to the best advantage; to require as little explanation as possible; and to be by itself as intelligible as possible.

If dissected out neatly and cleanly at once, free from extraneous, adipose, cellular, and muscular substance, &c. much subsequent trouble will be spared, and time saved. Generally speaking, indeed, the suggestion just given cannot be too strongly inculcated. For the morbid part to become a good preparation, it should be put out of hand at once; and nothing should be left that ought and can be removed by the knife and scissors. Delay breeds neglect and forgetfulness; the nicer peculiarities of

the diseased part are forgotten; after a time it ceases to excite interest, as a confused mass putrefying, or bordering on putrefaction, it is a loathsome and worthless object; and thus in consequence of not having been finished at once, it ends in being thrown away; and an aversion too often is acquired, rather than a fondness for pathological anatomy.

The present remarks are applicable to all kinds of anatomical preparations, but more especially those intended to be kept in spirit of wine, which experience has proved is better adapted for the preservation of moist preparations than any other liquid yet tried.

The methods of proceeding in preserving preparations, must to a certain extent be modified according to the nature of the morbid parts, and agreeably to the intention of the anatomist.

If the diseased part is small, and it is wished to preserve its colour, as a portion of inflamed, or of ulcerated stomach or intestine, it should be immersed immediately in strong spirit; and instantly put up, as it is intended it should remain. After a month the spirit may be changed for fresh spirit, and the mouth of the vessel should be firmly secured. The blood in the part will thus be coagulated and preserved; the shape will be retained without unseemly distortions, which when once rigid are not easily removed; and the preparation is fit for the shelf of the museum without any further trouble.

Preparations of the brain, spinal cord, and nerves,

should be treated in the same manner, and so treated they are easily kept.

Thus also should be managed preparations of the eye, pleuræ, peritoneum, testes, and their tunics, and in fact all such parts as are either colourless, and therefore not requiring steeping in water, or are liable to be injured by that process, and by incipient putrefaction.

On the contrary, parts containing much blood, as the liver, kidneys, lungs, heart; or stained and discoloured with blood or bile, &c., as the blood-vessels and gall-bladder, &c.; or, smeared with a lubricating fluid, as the aspera arteria, primæ viæ, synovial membranes, &c., should be immediately well washed, and if practicable, in a stream of water. By persevering washing in *running* water, and gently pressing the part, treating it like a sponge, in a short time, as from ten to twenty minutes, most textures may be deprived of the blood which they contain, and of any secreted fluids peculiar to them. If leisure does not admit of such manipulation, and assistance is not available for the purpose, the parts may be suspended in water, and the hotter the weather is, the shorter should be the time of suspension, and the more frequently the water should be changed. In thus steeping, it is of importance to use a tall large vessel, full of water,—and to have the parts immersed only just below the surface. The rationale of this must be manifest to those who reflect on the subject, and take into consideration, 1st, that blood

and bile, especially the coloring matter of the former, is of greater specific gravity than water, and consequently has a tendency to subside rapidly and collect at the bottom; and 2dly, that the colouring matter of both, and more particularly of blood, in solution in water, has the power of combining with animal textures, and of staining or dyeing them. By using a tall vessel in the manner suggested, the colouring matter in dissolving, will rapidly quit the part immersed, and descend beneath it; and by employing a large quantity of water, the object of separation is more effectually attained, and very frequent change of water is so much the less necessary. Demonstrative proof may be easily obtained of the propriety of these directions, by reversing them, as by allowing the part in steeping to sink to the bottom, or by using a shallow vessel or a small quantity of water. In the former instance, the part will necessarily be immersed in a strong solution of the colouring matter, and will be intensely dyed; and in the latter, the effect will be very similar, presuming that the colouring matter is diffused through the whole of the water.* I may appear to lay too much stress upon these points: but, in reality, they are very important, and commonly not

* Vide note Vol. II, p. 194, for further remarks on the staining agency of the colouring matter of blood. Whether dissolved in consequence of incipient putrefaction in serum, or merely in water previous to putrefaction even in an incipient state, its dying power appears to be similar.

sufficiently attended to; I have known them neglected even by anatomists, not unaccomplished in the art of making preparations for the museum; and I also know how difficult it is to have them observed by novices in the art, for whom chiefly these observations are intended.

Immediately after the thorough washing, as soon as possible, if steeping be employed, the parts should be transferred from water to spirit. Before immersion in the latter, unless of peculiar delicacy they may be pressed gently, between folds of linen to remove the excess of water. A mixture of seventy parts of proof spirit, and thirty of water (rain, or distilled water should be used*) is well adapted, even in warm climates, for preserving the majority of moist preparations.

For the useful purposes of a museum, it is necessary that the part to be kept should be, not only carefully and neatly dissected out, but also carefully, and neatly put up, and *that immediately*, and as it is intended it should appear on the shelf. If this be neglected at the moment, the season for doing it in perfection is lost. A preparation crammed into a bottle, just large enough to hold it, or thrown into spirits in a large vessel, as is too often done, without

* If the water contain carbonate and sulphate of lime, which are of common occurrence in spring water, they will be precipitated by the spirits; the appearance of the preparation may thus be injured or the inside of the jar may become dull, and prevent a distinct view of the contents.

attention to suspending it in a natural way, that it may be properly seen, becomes (unless it be some very simple structure) misshapen, distorted, and confused. A preparation in such a state, it is very difficult afterwards to amend: this is well known to those who have had occasion to attempt the annoying task of endeavouring "to make something" of a preparation, perhaps highly interesting in itself, which has been thus neglected in the first instance.

It may perhaps be thought that the measure just recommended is not easily carried into effect; that to accomplish it much art and skill are requisite; and that glass vessels in plenty, and of various sizes, are indispensable.

This, fortunately, is not the case; very moderate skill is sufficient, such as every medical man ought to possess, and must possess, if he is fond of his profession, and only tolerably zealous in the pursuit of it. And instead of many glass vessels, one or two are amply sufficient for holding all the preparations a professional man is likely to be able to collect in one year in the course of his ordinary practice.

A glass vessel of the capacity of a gallon, with a large mouth closed either with a cork enveloped in cerate-cloth, or a glass stopper, is very convenient for the object in question. The preparation neatly dissected, may be advantageously attached by a thread, the outer end of which should also be smeared with cerate; or if the preparation is lighter than spirit, as a portion of lung containing air, the same

object may be effected by attaching it to a piece of glass.

In this way a great many preparations may be introduced into the same vessel,—indeed the vessel may be almost filled with them without detriment, provided each is free, and not pressed against by another, which is easily managed by using threads of different lengths. This method, it may be added, is particularly well adapted for sending preparations to England, on account of its economy, the little space required, and its security. Using it, there is no danger of the preparations being left dry and ruined by the capillary action of threads sucking up the spirit, and draining the vessel, the cerate preventing such action; nor is there any danger of atmospheric air finding admission, provided the glass stopple or cork with which the bottle is closed is covered with moist bladder firmly tied down, and smeared with oil when dry; nor of any material loss from evaporation. Here a caution may be given, that when bladder is thus used, the bottles should be placed out of the reach of rats, mice, and cockroaches, animals greedy of this membrane, and who attack it whenever it comes in their way.

Numbers written with a lead pencil on slips of strong paper, parchment, or wood, may be introduced with the preparations when they are numerous and there is any apprehension of mistake; which numbers will of course have reference to a descrip-

tive list, that should accompany the preparations to England.

Relative to the making of dry preparations in hot climates, it will be sufficient to offer a very few observations. The unexperienced in these climates may fancy the task in question exceedingly easy, from a common and erroneous association of the ideas of proximity to the sun and parching heat. They will find it, however, a more difficult labour than can be imagined *à priori;* and for this reason, that the connexion of heat, and dryness just now alluded to, is in most hot climates of rarer occurrence than the association of high temperature with a great degree of humidity. This latter happens when the wind sweeps on its way over a great extent of sea, and on its passage becomes loaded with moisture, as is the case with the S.W. monsoon along the coast of India, the S.E. or sirocco in the Mediterranean, and the sea breeze in the West India islands. During the prevalence of these winds, it is very difficult to dry any anatomical preparation, and impossible indeed, unless recourse is had to some helping circumstance, as exposure to the direct rays of the sun, or the dry heat of a charcoal fire. On the contrary, when the atmospheric heat is accompanied with dryness, as it always is, when the wind comes over an extensive tract of country, such as the land wind in India, the N.W. in the Ionian Islands, the S.E. on the Western shore of Southern

Africa, then the making of dry preparations is very easy,—exposure to the wind is by itself sufficient. When dry, in every instance, the preparations should be varnished to defend them from the action of the atmosphere, and from the effect of vicissitudes in point of humidity, and then they should be carefully packed up in dried paper in a box of tight construction to be sent home by the first opportunity. As dry preparations of morbid parts with the exception of bones, are of comparatively little value, nothing that is particularly interesting, capable of being kept in spirits should be preserved in any other way.

It is not considered necessary to give any hints respecting the methods of making injected preparations. Those who have sufficient zeal for anatomical pursuits to engage in this undertaking, will have no difficulty in carrying it into effect wherever they may be, with the knowledge which they must have acquired previously, and being possessed of the same apparatus that is requisite in temperate climates.

In concluding, it may be remarked, that, simple and easy as are the means described for making and preserving morbid anatomical preparations in hot climates, they are quite adequate, and with ordinary care cannot fail to succeed. On this subject I can speak with some confidence from experience. During a period of eleven years, that I was stationed in the Mediterranean, in the Ionian Islands

and Malta, they had a fair trial, and were found to answer perfectly; of which, I may add, permanent proof is afforded by the specimens of diseased structure thus preserved, sent from thence, and which are now in the Museum of Pathological Anatomy at Fort Pitt.

XXIV.

NOTICE OF A CASE IN WHICH THE ARTERIA INNOMINATA AND THE LEFT SUBCLAVIAN AND CAROTID ARTERIES WERE CLOSED WITHOUT LOSS OF LIFE.

Sir Astley Cooper, in the first volume of Guy's Hospital Reports, has given a valuable paper on the effects of tying the carotid and vertebral arteries in the dog and the rabbit, proving, that in the former animal both arteries may be suddenly and at the same time tied, without destruction of life, and that in the latter they may be successively tied with the same result,—the circulation of the blood in the brain, being continued by the admirable conservative process of anastomosis.

What Sir Astley Cooper has demonstrated as practicable in these animals, might be inferred analogically of man, taking into account the results of ample experience on the ligature of all the larger arteries singly in different instances for aneurism, successful beyond even sanguine expectation; and a case which has come under my obser-

vation, confirms the justness of the conclusion, as clearly I apprehend, as if instituted, were it possible, experimentally for the purpose. As the subject is very important in its physiological relations, as well as in its bearings on pathology, I shall describe this particular instance with some minuteness, although not so much in detail as may be desirable, owing to circumstances connected with the individual.

An officer of rank, aged about 55, distinguished for his services in the field during the Peninsular war, and who had been severely wounded in the chest at Waterloo; after good health for several years, had, when absent in England from his command in the Mediterranean, in the winter of 1831, an attack, which I was informed was considered rheumatic, attended with pain in the right shoulder, for which calomel and opium were largely prescribed. In September of the following year, he had acute catarrh, of considerable severity and duration, followed by chronic inflammation with enlargement of the uvula and a granular state of the pharynx, of great obstinacy, attended with cough, and an impaired state of the general health. In the spring of 1834, when his health appeared to be improving, he noticed a peculiarity of alvine evacuation; and scrutiny being made, it was found, that he voided daily a small quantity of purulent fluid, and which resisted treatment for rather more than six weeks. The summer of this year was

spent in Switzerland, where his health improved. During the winter of 1834-5, he was in a valetudinary state; subject to cough; to frequent pain in the right shoulder; to occasional difficulty of breathing: the pulse was small and always quick; rarely below 90°,—sometimes exceeding 100°: the respiratory sound was natural; the heart's action (judging from its sound and impulse) was rather tumultuous: there was emaciation, loss of strength, and depression of spirits,—alarming his friends. For several months, his state fluctuated,—on the whole apparently improving. He ceased to be immediately under my observation in March 1835,—on my leaving the Mediterranean. In the June following, he returned to England by way of the Continent; and in August, about three weeks after, he wrote to me, that he was then unusually well, but that on landing, he had been ailing a little, and had consulted Dr. Chambers; and this gentleman in a note with which he has favoured me, in reply to my inquiry relative to the then state of pulse, mentions that he "never could perceive any pulse from the time he first saw him." In September, he went to Brighton, and was taken suddenly and seriously ill with tendency to syncope and vertigo frequently recurring: then, it is certain that no pulse could be perceived at either wrist. In October, when he had returned to town, I saw him twice, and the second time in consultation with Dr. Chambers and Dr. Hume. His general health was better than it had

been at Brighton; he experienced vertigo seldom, and syncope never; he daily walked to his club-house in St. James' from an adjoining street, and played gently at billiards under caution. His disease now was clearly aneurism of the arch of the aorta, with an obstructed state (it might be inferred) of the great vessels arising from it; for no pulse could be felt any where in the course of these vessels, neither in the neck, temples, axilla or wrists, and there was a throbbing pulse at the upper part of the sternum, and a slight prominence of the bone there to some little extent. Until the autumn following, I saw him generally once a month: he had fewer uneasy sensations, — felt stronger and considered himself better; the rising of sternum was rather extending, the abnormal pulsation not increasing. He then went into the country; he felt, I was informed, still better; his general health was decidedly better. This was indicated by his wish to shoot,—an amusement he was extremely fond of, but which, when applied to, of course I opposed; strongly urging in common with his ordinary medical attendant, his remaining very quiet, and in regard to diet living below par. On the 11th of January, 1837, that is, nearly a year and a half from the cessation of the pulse at the wrist, he expired suddenly. That morning he left his friends to return to town, and felt and appeared unusually well: he travelled about 60 miles in a close carriage before stopping to dine, and where he intended to pass the night. The last

post or two, he felt some pain in his shoulder, but, as I was informed, spoke lightly of it. According to his usage on quitting his carriage, he took in his hand a small writing case; and shortly after arrival at the inn, sat down to dinner with an appetite, and the expectation of the removal of some uneasy feelings. He took some soup, and was in the act of taking some fish, when suddenly his jaw was observed to drop, and without uttering a word or a sound, he fell back in the chair dead.

A *post mortem* examination was made by a very competent surgeon. The pericardium was found distended with coagulated blood, and a small fissure was detected in the aorta, near its base, through which the blood had penetrated. The heart was rather large, and its cavities large, but without distinct hypertrophy; its valves natural. The arch of the aorta was, as had been inferred, the seat of a large aneurism, about six inches high, and about four inches wide, nearly filled with coagulum, dense, fibrous, and nearly white, occupying all but the inferior space, where there was a channel open by which the blood passed from the heart into the descending portion of the vessel. Where the vessel commenced its descent, there it recovered its natural dimensions; but though of ordinary size, it was not perfectly sound; there were some atheromatous deposits on it, and a few bony scales. All the great vessels rising from the arch were completely closed up at their origin. The upper portion of the in-

nominata was open; the right carotid and subclavian arteries also were open, but rather diminished in size. The left carotid, subclavian and vertebral arteries, as far as they were examined, namely to the extent of about two inches, were impervious, plugged up with lymph. The intercostal arteries at their origin (they were not examined farther) were large. The state of the cerebral vessels, in consequence of the rapid manner in which the inspection was made, was not ascertained: the aneurismal tumor was preserved, and through the kindness of the gentleman, who removed it, (Mr. Johnston,) I had an opportunity of examining it, and of satisfying myself respecting the most important points.

In this instance, probably, the aneurism was of slow formation, and was latent, as so often happens in this malady, not only many months but even several years: the interesting part of the case, however, is the obstruction of the great arteries, arising from the diseased portion, at their origin, and yet the supply of blood, not being cut off, clearly proving, that new channels were established by anastomoses, by which a quantity of arterial blood was transmitted to the head and upper limbs, not only sufficient to maintain life, but enough to maintain it with the vigour of ordinary health. At no time, after the obstruction of the great arteries was there any paralytic weakness of the hands, no numbness even of the fingers, and no decided diminution of temperature. The intellectual organ performed its

functions unimpaired, excepting there was occasionally some slight confusion of mind and a vertiginous feeling, especially in suddenly rising from the horizontal or reclining to the erect posture. All the senses were ordinarily unaffected: the sight, the hearing, the smell, and taste, were commonly as acute as ever, and the general functions of the system were well performed; so that, had not the aneurism burst into the cavity of the pericardium, life might have been continued with ease and enjoyment, and no doubt would have been so continued, had a channel for the passage of the blood been formed through the fibrinous mass in the aneurism instead of below it.

As the circumstances under which the post-mortem inspection was instituted did not admit of minute examination, much less of the injection of the vessels, the manner in which the circulation was carried on above the obstructed vessels can only be conjectured. The intercostal arteries, especially the superior, were probably the ones chiefly concerned in effecting a communication, either with the pervious principal arteries springing from the innominata, and their branches, or the probably pervious branches, given off by the left subclavian and carotid arteries.

As supplementary to this case, I shall briefly notice another, in which also there is reason to believe, that the same great vessels were permanently obstructed, as in the preceding, but with a train of symptoms and an event very different.

Hector M'Callum, of the 42nd regiment, aged 36; twenty-two years a drummer in that corps; notwithstanding intemperate habits, had good health, until the beginning of 1833; when he first experienced some difficulty of breathing; but he continued at his duty until the 18th of February 1834. He was then admitted into hospital, at Malta, and I frequently saw him in company with the able surgeon of the regiment, Dr. Nicholson, (now Assistant-Inspector of Hospitals,) to whom I am indebted for most of the particulars of the case. The principal symptoms at that time were palpitation with tumultuous action of the heart, greatly aggravated by bodily exertion, with dyspnœa, aggravated by the same cause, occurring in paroxysms, during which he experienced temporary loss of vision, and occasionally syncope. No pulse could be felt at either wrist, nor in the brachial arteries, and indistinctly only in the carotid arteries, but strong in the femoral. The tongue was clean, the appetite good, the functions generally (independent of respiration) well performed; yet he was feeble. He gradually became worse. In May, the dyspnœa was continually severe, often threatening suffocation, the face assuming a livid hue, with great agony of suffering. Now, no pulsation could be felt in the left carotid artery, and only very indistinctly in the right, without return of the pulse in either wrist. He died suddenly on the 2d of June.

Owing to aversion on the part of the widow to a

post-mortem examination, the inspection was very partial and hurried.

Two pints of serum were found in the left pleura and three in the right. The lungs were not apparently diseased. The heart was somewhat larger than natural: its valves were pretty sound, even the semilunar of the aorta. The aorta throughout its arch was enlarged; but only in a moderate degree, and its coats were more or less altered. Its inner coat was irregularly thickened and opaque, and the middle coat corresponding had become thinner, and of a heightened yellow tint. Thin plates of bone were formed in, or under the former, and atheromatous matter here and there in the latter, and in one spot matter of this kind, of about the bulk of a peppercorn, soft, brownish, poultaceous, had penetrated through all the coats to the external loose cellular enveloping tissue. The left carotid and subclavian arteries were completely obstructed, firmly plugged up with dense white matter, which it may be inferred was lymph. No trace even of the opening of the carotid from the aorta could be discovered, and a very slight trace only of the other, a little cavity a line or two deep, resembling the vestige of the aperture of the ductus arteriosus in the adult. It is to be regretted that in the hurried examination, and owing to interruption, the state of the arteria innominata, and of the right subclavian and carotid arteries was not determined. The descending aorta was of its normal dimensions.

This second case in relation to symptoms is remarkably contrasted with the first :—in the one, a natural curative process appears to have been set up, to counteract the local evil; in the other, the reverse of this process appears to have been in progress,—probably a closing of the larger arteries, without expansion and extension of their branches, or of the adjoining arteries, and consequently the production of a permanent impediment to the circulation in the brain and upper limbs, and especially in the former. If this were so, fortunately, it may be considered a very rare occurrence, and as it were, the exception to the general rule. The common train of events, that which constitutes the rule, seems to be that which was witnessed in the first instance, and which has been so happily illustrated by the experiments of Sir Astley Cooper already referred to.

The subject of the occlusion of arteries is one of great interest and of more frequent occurrence, I apprehend, than is commonly supposed. I have met with many examples of it, after death, which during life were never suspected, and even in large vessels, and in soldiers of active habits, who died of acute diseases, of short duration totally unconnected with the local peculiarity. I am disposed to believe that whenever the middle coat of an artery becomes diseased, whether from atrophy, or change of structure, or deposition in it, of that matter which has been called atheromatous, or any

other, there is invariably an effort, as it were, made, to strengthen the part, by the deposition of lymph on both sides; and that according to the rate of progress of the two processes, there is either a closure of the vessel, or an aneurism established: if the lymph is deposited rapidly, the former; if the athermatous matter, the latter; and also the latter, if the absorption or weakening of the middle coat from diseased alteration in its texture proceed with more speed than the deposition of lymph. This, I admit is hypothesis; I wish it to be considered as such; I venture to propose it, with the hope of calling attention to a subject, as I believe, of great importance, and with the hope of exciting discussion, which is always useful.

XXV.

NOTICE OF A FATAL CASE OF RUPTURE OF THE HEART AND AORTA; WITH AN ACCOUNT OF SOME EXPERIMENTS ON THE POWER OF RESISTANCE OF THE HEART AND GREAT VESSELS.

Thomas M'Garey, aged 42, about to be invalided on account of length of service and diminished activity; according to a comrade, of "lonely habit," fond of walking by himself; but well conducted and sober, and previously in good health,—was found dead early in the morning of April 14th, 1823, in a chalk-pit in the neighbourhood of Chatham, at the foot of a precipice, between 50 and 60 feet deep,—the greater part of it, perpendicular. The body was examined on the 16th, the weather was cool, and it had been kept in a cool place. Externally there was no mark of violence or appearance of contusion. The face and surface of the body generally were pale. The pupils were contracted; the limbs rigid. The right inferior extremity was a little shorter than the left; it was supposed that the

thigh was fractured; and on exposing the bone it proved to be so, at the neck, in an oblique direction just without the capsular ligament. Nothing morbid was discovered in the brain. No ribs were fractured. On opening the chest, a large quantity of blood was found in the left pleura, about six pints, and a small quantity in the pericardium, about an ounce. The blood was dark, of the colour of venous blood, and had separated into crassamentum and serum, and the serum was tinged red. No lesion was detected either in the right auricle or ventricle, or in the vena cava ascendens or descendens, or in the pulmonary artery. The left ventricle also was uninjured; but not so the left auricle and aorta, each was ruptured. The true auricle was ruptured in two places; each opening was sufficiently large to allow the passing of the finger. The aorta was ruptured just above the mouths of the coronary arteries; and also at the upper part of its arch, a few lines inferior to the origin of the left carotid and subclavian arteries. The laceration at its base was nearly circular, and through all the coats, and were it not for a narrow strip, of about two or three lines which remained entire, the vessel had been completely torn in two; even the external loose cellular membrane was divided to the extent of about half its circumference. The rupture at the arch was oblique, also through all the coats, and rather more than one half the circumference of the vessel. There was no blood

in either of the cavities of the heart. Both ventricles were firmly contracted ; and the valves of the aorta and pulmonary artery were unusually large and perfect. The aorta was carefully examined throughout its course; excepting two or three opaque spots on its inner membrane, without sensible thickening, it exhibited no signs of disease, and appeared to be of usual firmness. Where the vessel was ruptured, in each place the coats seemed sound; nor did the ruptured auricle exhibit any indications of organic disease. The lungs were free, and crepitous; their substance was redder than usual, and contained more blood than usual. The trachea and bronchia were bright red internally, and coated with blood, probably owing to hemorrhage from the lungs. Nothing abnormal was found in the cavity of the abdomen; neither the spleen nor liver, (of all the viscera most liable to laceration from falls) was appreciably injured. Both the stomach and bladder were found moderately distended, one with yellowish chyme, which had no smell of spirits,—the other with pale urine.

According to the report of those who discovered the body, and of those who saw it before it was moved, it lay on its back, — the feet nearest the precipice, in a diagonal position. The limbs, it is said, were rigid and every part of the body cold, excepting the left side of the chest which was pretty warm. No part of the dress was torn. The ground was firm, bare chalk.

These particulars of this very uncommon case, were noted down at the time. Taking them all into consideration, it seems most probable, that the soldier lost his way in a lonely night-walk, and coming to the brink of the precipice stepped over and fell on his feet, or rather perhaps, on the one (the left) the neck of the thigh-bone of which was fractured. On this supposition the unbruised state of the integuments, the uninjured state of the clothes, and the oblique or diagonal position of the body in relation to the foot of the precipice, as well as the kind of fracture, seem to be tolerably accounted for.

Relative to the fatal injuries, the rupture of the left auricle, and the double rupture of the aorta,—I apprehend, that, reasoning *à priori*, conjectures only can be offered as to the manner in which they were produced; and especially taking into account a fact, which occurred about the same time, and of the accuracy of which I am well assured, viz. that a lad fell down the same precipice and alighted on his feet without sustaining the slightest injury. But, however explained, whether on the supposition that the lacerations were owing to muscular action,—to the violent contraction of the left auricle and ventricle; or, to other causes in conjunction with such action,—the effects themselves seemed to demonstrate, that where they took place, there was a greater tendency to yield and to rupture than elsewhere, at least in this particular case.

Thinking it probable that elucidation might be

obtained by experiment,—I instituted at the time some trials directed to test the strength of the parts concerned, which I shall now detail, with a few additional ones since made.

Experiment 1.

Tied the descending vena cava of a sheep recently killed, and through an opening in the ascending vena cava, introduced the tube of a powerful injecting syringe, and secured it firmly by ligature. Forced water into the right side of the heart; a large quantity was introduced; the vein, auricle, ventricle and pulmonary artery became very much distended, as did also the lungs, and a watery exudation took place from their surface, without any sensible breach of continuity of surface. Continuing the forcible injection of water, the right side was at length ruptured; the rupture took place in the sinus venosus close to the auricle, and was large enough to admit the finger.

Experiment 2.

Made a similar trial on the left side of the heart of the same animal, by injecting water through one of the pulmonary veins, close to its termination, having previously secured by ligature the aorta, below the arteria innominata. The auricle became greatly distended, the ventricle in a less degree, and the aorta still less. Rupture took place in this instance much more readily than in the former; the

auricle was the seat of it; it was sufficiently large to admit the finger.

Experiment 3.

Made a similar experiment on the aorta. It expanded but little; it was ruptured about one inch from its origin, and to the extent nearly of one half of its circumference. The pipe of the syringe was introduced just below the arteria innominata. The semilunar valves performed their function perfectly, and were found uninjured.

Experiment 4.

Made a similar experiment on the pulmonary artery. Introduced the pipe of the syringe into one of its main branches, and secured the other by ligature. It dilated more than the aorta, and considerably; its rupture took place before much force had been employed; it was transverse, about one half the circumference of the vessel, and through all its coats, and about half an inch above the valves, which acted well and were uninjured.

Experiment 5.

Tied the pulmonary artery, and descending vena cava, and forced in water through the ascending vena cava: when but little force had been used, and when the auricle and ventricle were not very much distended, a rupture took place, which on examination was found to be of the fossa ovalis; the

rent was oblique; the membrane was torn at its margin, with some muscular fibres with which it was there united.

Experiment 6.

Tied the ascending vena cava, just before its termination in the heart, and having introduced the pipe of the syringe, through a small opening made in the vessel just above the liver, and secured it well by ligature; an attempt was made to burst it, but in vain, although all the force of a strong man was applied to the piston, aided by all the pressure I could make with the hand on the dilated vein: its dilatation was considerable, but not to the extent that might have been expected.

Experiment 7.

This and the following experiments were made on the human subject. About four hours after death, from tubercular disease of cerebrum, with softening of its substance, complicated with tubercles and a few small cavities in the lung, in the instance of a young soldier, aged 26, water was injected through one pulmonary vein, close to its termination;—the other pulmonary veins, and the arteria innominata, and the left carotid and subclavian arteries, as well as the aorta just below, having been secured by ligature. The left side of the heart became greatly distended: after much force had been applied a rupture took place—the auricle, its sinus, burst

close to the entrance of one of the pulmonary veins.*

Experiment 8.

About four hours after death from chronic dysentery, complicated with a few tubercles in the lungs, and two small cavities, in the instance of a soldier aged 28 years, after tying the aorta below its arch

* In the above, and in many other instances noticed in this work, the post-mortem examination was made sooner than is usual. In all such cases means were taken to determine previously the reality of the fatal event—as by making a small incision through the cutis, and puncturing the fibres of the platisma myoides (a muscle, which is probably very retentive of vital contractile power, judging from the analogy of the panniculus carnosus) and by exposing the eye to light. To the pathologist, conversant with necroscopical research, such precautions may appear superfluous; he may say, Who ever knew an example in hospital experience, of a body supposed to be dead, and removed as such from the ward to the dead-house, either reviving, or affording any indications of remaining vitality? I never witnessed an example of the kind, nor ever heard of one well authenticated, excepting in bodies which had died of that mysterious disease, cholera, in which in a few rare instances, it is well established, that particular muscles have acted, after the extinction of life. Notwithstanding, although all experience supports the foregoing conclusion, yet as apparent death—suspended animation is within the limits of possibility, and keeping in recollection what happened to the illustrious Vesalius, it is at least in accordance with proper feeling, and a satisfaction to employ the tests alluded to; and the popular advocates of measures to prevent what is held up to horror—too early burying, on the supposition combated, that suspended animation and apparent death is not uncommon, would act more consistently with their views, if, instead of proposing that bells should be hung up within reach of the interred, or attached to their persons, they recommended the puncturing of some irritable part, before the body is committed to the coffin.

and its main ascending branches, injected water by one of the pulmonary veins, the others being left untied. The left auricle became exceedingly distended, and the coronary arteries. The ventricle and aorta were less distended. The lungs were very much distended, and there was an oozing of water from their surface; when a portion was pressed gently between the hands, the water actually dropt from it, and yet the investing pleura, appeared to retain its integrity. After employing much force, a rupture was with some difficulty effected. The water rushed out through the trachea, indicating the bursting of a pulmonary vein in the substance of the lung.*

Experiment 9.†

Next secured by ligature the pulmonary veins,

* It was curious to see in this instance, how the lining membrane of the left side of the heart, and of the aorta, above the ligature, were intensely stained red; whilst the portion of the latter below it, was colourless; affording strong proof of the dyeing power of the colouring matter of the blood (the left side of the heart contained some blood) when in solution.

† Previous to commencing this experiment, a small opening was made in the pericardium, and the contained fluid was carefully removed by means of a sponge, and then the opening was closed by a ligature. At the end of the experiment, it was re-opened and examined; notwithstanding the distended state of the auricle and ventricle, and of the coronary arteries, and that nearly half an hour was passed in making the trial, owing to delays, not more than three or four drops of water were found collected, the result of exudation. This result may be adduced as a confirmation of the conclusion arrived at, page 239, Vol. II., relative to the source of the fluid so frequently found in the pericardium after death.

just before their termination, and the syringe being applied to the same vein as in the former instance, the subject being the same, and the other circumstances the same, water was injected, until a rupture occurred. It took place opposite the fossa ovalis, in the thickest part of the sinus, close to the auricular-ventricular passage; the rent was sufficiently large to admit two fingers. Much force was required to effect it, and it was preceded by much dilatation.

Experiment 10.

From the pulmonary vein, the pipe of the syringe was removed to the descending aorta, just where it begins to descend, and was secured by ligature. On injecting water, the vessel gradually became much dilated; at first the semilunar valves appeared to perform their functions well; but as the dilatation increased, water began to flow out through the ruptured auricle, augmenting with the distension, till it flowed freely.

Experiment 11.

Twenty-nine hours after death, from pulmonary consumption with numerous cavities in the lungs, and one of vast size, in the instance of a soldier aged 29; introduced the pipe of the syringe into the right internal jugular vein, not having tied any vessel, excepting the opposite veins in the neck,—the cavity of the abdomen having been carefully laid

open, and also of the chest partially by the removal of the sternum and of the costal cartilages. After a certain quantity of water was injected, it flowed out through the trachea, probably through ruptured vessels in the tubercular excavations. The lungs, the liver and spleen were rendered tense, and there was an exudation of fluid during the continuance of the pressure, not only from the surface of the lungs, but apparently, generally, in the cavity of the abdomen from all its contained viscera.

Experiment 12.

The result was similar, when water was injected, into the aorta, just above its division into the common iliac arteries. As the artery became dilated, the semilunar valves performed their function less perfectly, and the flow of water through the trachea increased.

Experiment 13.

Twenty-five hours after death from peritoneal inflammation, in the instance of an insane soldier, aged 44 ; after examining the brain, and securing the vertebral arteries within the cranium, and the carotid arteries in the neck, previous to opening the chest ; introduced the pipe of the syringe into the aorta just before its division into the common iliac arteries, and having secured it by ligature, (all the abdominal viscera remaining undisturbed,) injected water as before. It was necessary to inject a very large

quantity, and to employ great force, before a rupture was effected. The ruptured part was detected with some difficulty; it proved to be the left emulgent artery, about a quarter of an inch from its origin. Its middle coat was lacerated in two places, in the direction of the fibres composing it, and also the inner coat. There was no perceptible opening in the outer cellular. The course of the aorta, even through the chest, was distended with water, and indeed the whole of the posterior mediastinum was so distended, and pretty much had found its way into the pleuræ. The left ventricle and auricle were empty, excepting that the latter contained a little blood, shewing that the semilunar valves had performed their function to perfection.

Experiment 14.

Thirteen hours after death from tubercles and cavities in the lungs, complicated with diseased omentum, enlargement of the liver and severe ulceration of the large intestines, in the instance of a soldier aged 33; after examining the brain and tying the jugular veins, the pipe of the syringe was introduced into the right internal jugular vein, below its ligature, and water was injected; considerable resistance was almost immediately experienced, giving the idea, that the right cavities of the heart were obstructed with coagulated blood. After the syringe had been emptied two or three times,

great force was required to introduce more water; the vein towards the heart was amazingly distended, and shortly the pressure being continued, it became ruptured; the effect took place just before the entering of the vessel into the anterior mediastinum. It seemed as it were rather the separation of its fibres with distension, than the breaking of them from distension.

Experiment 15.

The vertebral arteries, where they enter the cranium having been secured by ligature, and the left carotid artery in the neck having also been tied, the pipe of the syringe was placed in the right carotid artery, and water was injected,—the cavity of the chest and abdomen remaining unopened. Very little resistance was experienced till a large quantity of water had been injected, and even when an enormous quantity had been introduced, the resistance was not considerable. The experiment was discontinued when the flow of water from the neck, the nostrils and the inside of the cranium, was almost equal in quantity to that which could be thrown in. This flow of water seemed to be from exudation. The body was amazingly distended, and exhibited a very singular appearance,—the penis turgid, the mamillæ dilated, their papillæ erect, the trunk and limbs, tense, rounded and filled out, which, (with the exception of the abdomen in which some fluid was effused in

connexion with the diseased omentum and enlarged liver,) were previously shrunk and emaciated.*

Experiment 16.

The sternum having been removed and the pericardium laid open, water was again injected into the right carotid artery, other circumstances remaining the same. The arch of the aorta, as much as could be seen of it, was greatly distended: but the left ventricle was not at all distended. No force that could be used could rupture the aorta,—the exudation continuing. Laid open carefully the left ventricle so as to be able to apply the end of the

* Hales, in his Hæmastatics, details many curious and instructive results of experiments on the injection of water into the vessels. In an experiment on a dog, he states: "If the warm water was continued thus" (in a column $9\frac{1}{2}$ feet high) "flowing into the artery for half an hour, or two hours, all the parts of the body, would, during that time, be continually swelling bigger and bigger, so that there would be an universal dropsy over the whole body; both the *ascites* and the *anasarca:* the salival and other glands were greatly swelled, and the mouth and nose filled with mucose slimy matter, which flowed from those glands; the ubera were much distended by the filling of their fat vesicles, as were also all the fatty vesicles of the body. All the muscles were swelled, and the interstices of their fibres filled with water; and some of them were by this means washed white. All this was effected with a force of water no greater than that of arterial blood in its natural state."—Stat. Essay, 3d ed. vol. ii. p. 113. With these results, the above, and what else was observed at the same time, so accorded, that in reading the one, I found described the other, substituting perhaps for fatty vesicles when speaking of the ubera, their erectile vascular tissue, and for the same term generally, cellular tissue, or the subcutaneous vascular.

finger to the semi-lunar valves. Water was again injected forcibly, the aorta was again distended, and there was only an oozing of water from the ventricle: the valves were felt very tense, and, it may be inferred, completely closed the mouth of the vessel.

Experiment 17.

I shall give one trial more, in which—twenty-four hours after death from tubercles in the lungs, with very large cavities in the left lung and extensive ulceration of the large intestines—the pipe of the syringe was introduced into the right carotid artery, before the calvaria was removed, and after tying both jugular veins, and both vertebral arteries, close to their origin, and the left carotid in the neck. As water was injected, all the integuments of the neck, face, and head became distended, especially of the right side, and at length, in a very remarkable manner, much surpassing the greatest swelling I ever witnessed in the most severe cases of erysipelas, or of confluent small pox. The eye-lids, nose, lips, were protuberant and tense; the eyes closed; the mouth open, from expansion of the lips; the tongue enlarged and tense. Water exuded from the eyes, the nostrils and mouth; and, it would appear, from their surface generally. The experiment was discontinued, when the water which flowed out from various parts seemed to be nearly equal in quantity to that which was injected. On examination no large vessel could be found ruptured. On removing

the calvaria, a strongly marked contrast was observable between the state of the exterior parts and of the interior, as, perhaps, might, *à priori*, have been expected, considering how the latter were encased in unyielding bony parieties. There was only a very little fluid effused in the cellular tissue of the pia mater; merely a moisture between the arachnoid membrane and the dura mater, and only a moderate quantity in the lateral ventricles and at the base of the brain: and the fluid so effused was probably not water which had been injected, but which had previously existed there; this is inferred from the circumstance, that there was a larger proportion in the left lateral ventricle than in the right, notwithstanding, that the pia mater of the former side throughout was red,—its vessels containing blood as usual, whilst that of the right was perfectly colourless,—all the blood having been washed out of its vessels. It is hardly necessary to observe that no vessel was found ruptured within the cranium.

The results of these experiments, in the way of illustration, as applied to the case with a view to which they were instituted, are in no wise clear and satisfactory; but, on that account I have not thought it right to keep them back, believing that as they have been faithfully detailed, they may be not altogether unacceptable or without use, especially in connexion with the important and obscure subject of aneurism.

Considering them generally, they are, perhaps, less uniform than might have been expected; and hardly favourable to any general conclusions being deduced from them, excepting, indeed, the following:—1st, That the power of resistance possessed by the heart and large vessels, independent of any vital properties of endurance peculiar to them, is enormous;* and, 2ndly, that there is much variation in point of strength in the same parts in different instances.

I have alluded to the rareness of the accident, which induced me to institute these trials. Referring to authors, I can find only one clear instance on record of a similar kind, that is, of rupture of any part of the heart, from a force acting through the medium of other parts, on this organ, apparently free from disease and in its perfectly normal healthy state. The case is noticed by Portal, in his Anatomie Medicale;* it was of a young man, over part of whose chest, the wheel of a cart loaded with stones

* Vide Hale's Hæmastatics.—The results of his experiments on the above point are very striking. Referring to them, he says,—"We see in these instances, the great strength of the coats of these vessels; what great reason have we, therefore, with thankful hearts to say to our Creator, as holy Job did, when he contemplated on the wonderful frame and strength of his body, Job x. 11, *Thou hast* not only *fenced me with bones and sinews,* but hast also effectually secured the vital fluid in such strongly wrought channels as are proof against its most lively and vigorous sallies, when either agitated by the different passions, or by strong and brisk actions of the body."—Op. Cit. vol. ii. p. 156.

† Tom. iii. p. 97.

passed: death was almost immediate, and the left auricle was found open. Chaussier, who examined the body, attributed the effect of the rupture to compression of the aorta by the wheel, and to over-distension of the auricle with blood, the consequence of that obstruction. It is true that very many other cases of rupture of the heart are recorded,—as accidents,—but they are either not sufficiently clearly described, to be depended upon, or there is reason to suppose that the heart was either crushed or else wounded, or that the part ruptured was previously diseased. The authors I have chiefly consulted have been, Senac, Morgagni, Haller, Portal, and Laennec. On the comparative strength of different vessels, besides Hale's very ingenious work already referred to, I have consulted Clifton Wintringham's "Experimental Inquiry on some parts of the Animal Structure," published in 1740, a few years after the Statical Essays of the former; but although having great pretensions to accuracy in his manner of giving the details of his experiments, I must confess I have not been able to place sufficient confidence in his results to use them.

In the Second Volume, a table will be found of the specific gravities of different textures, formed from experiments conducted with some care, and which I believe to be tolerably accurate. It was in comparing some of my results with his, that my faith in his correctness was shaken. According to him (to mention only one point), there is a marked

difference in the specific gravity of the descending aorta and of the vena cava corresponding, gradually diminishing with age: thus, the specific gravity of these vessels in a young man appears from his details to be, as 1060, and 1101,—the former the artery, the latter the vein; and in an old man as 1098 and 1105.

This difference I have not been able to discover. Very recently, I have carefully tried the specific gravity of each vessel, belonging to a man aged 30; —the artery was 1060, the vein 1061. They were similarly treated in every respect preparatory to weighing.

Clifton Wintringham's results are so uniformly consistent,—they all support each other so well, and are in such perfect conformity with his conclusions, that it is rather difficult to avoid the inference that he formed an hypothesis in the first instance and then made experiments to support it,—notwithstanding the passage from Leibnitz inserted in the title page of his book—*Ex iis, quæ experimentis constant, ducere tentimus quicquid potest, antequam in hypotheses liberiores expatiemur.*

END OF VOL. I.

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Fig 1.(B)
Fig. 1
Fig. 2
a
b
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Pl 2

Fig 1.

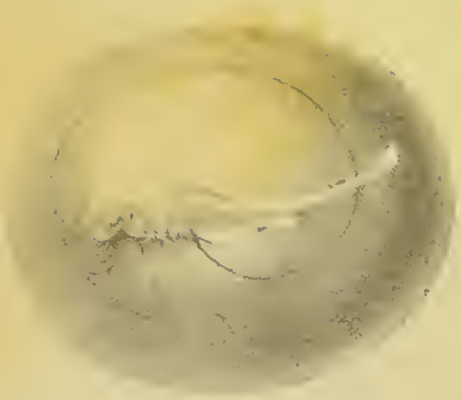

Fig 2

Fig. 3

Fig. 5

Fig 4

Pl IV

Fig.1

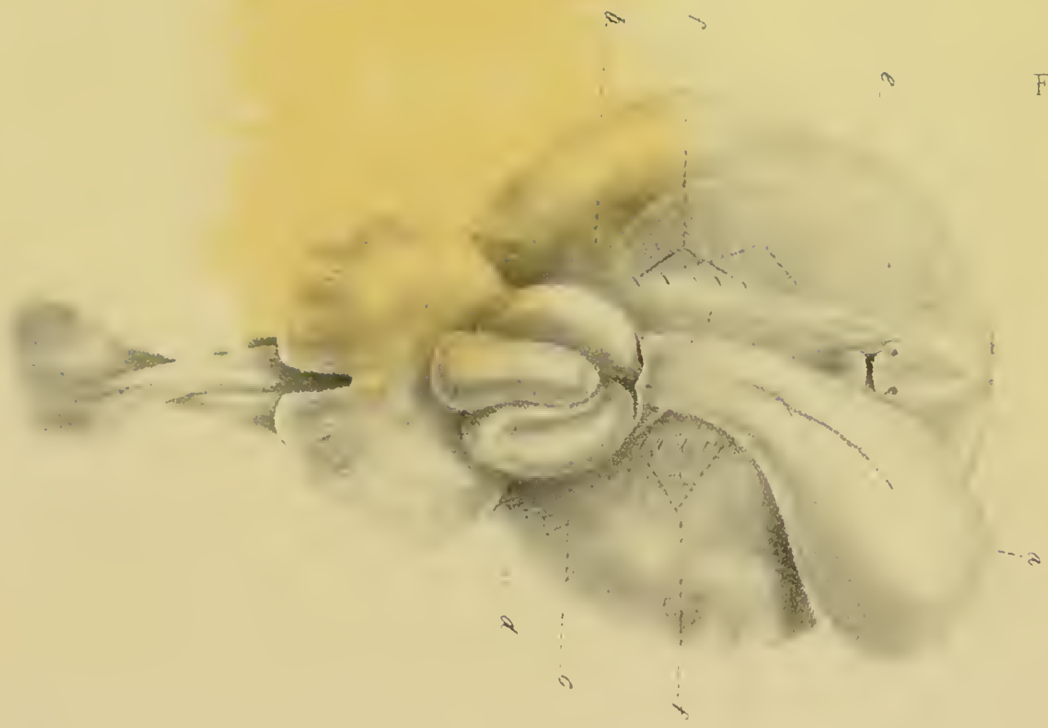

Fig. 2

Fig 2.

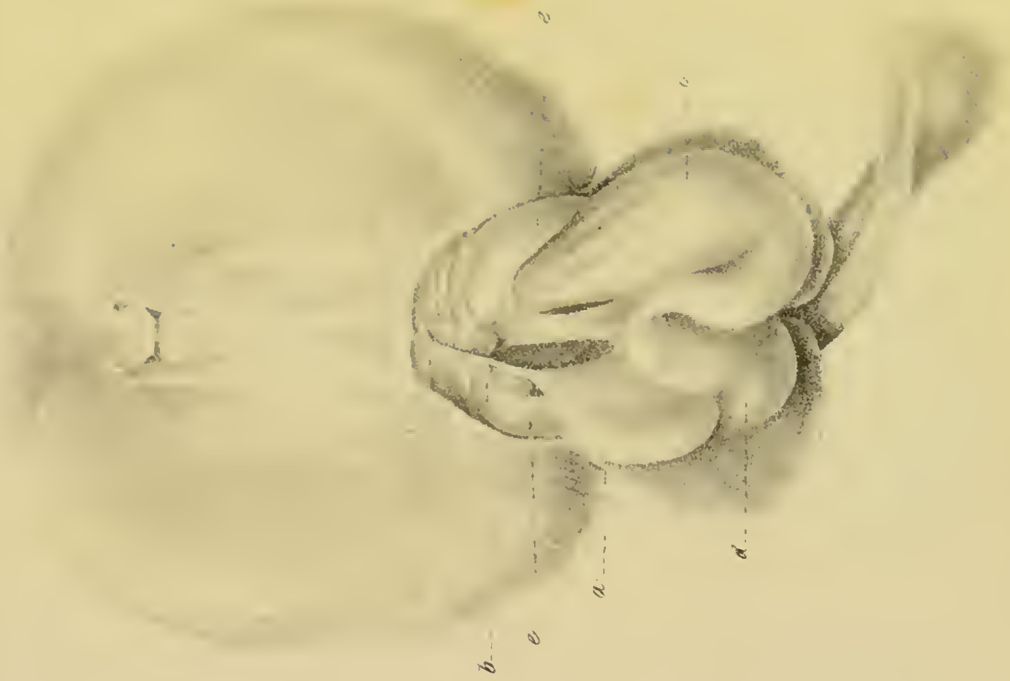

Fig 1.

Fig 1

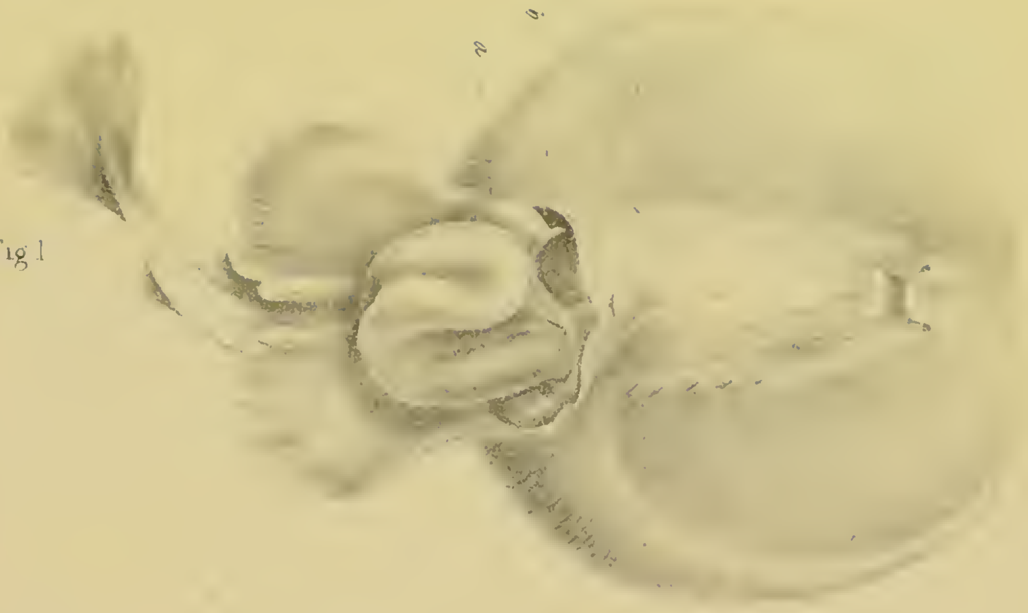

Fig 2

Plate VII.

Fig. 1.

Fig 2.

Plate VIII

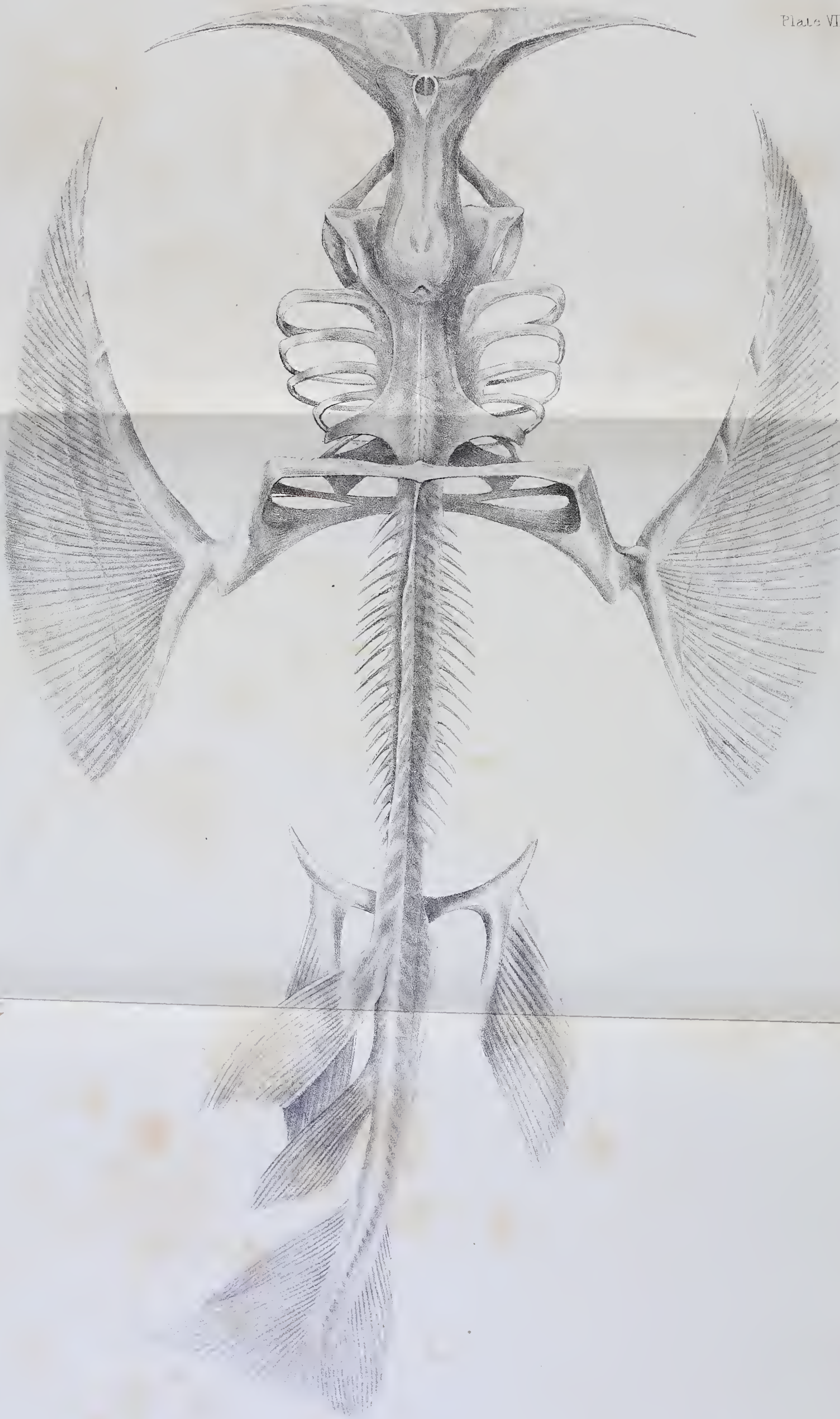

Plate IX

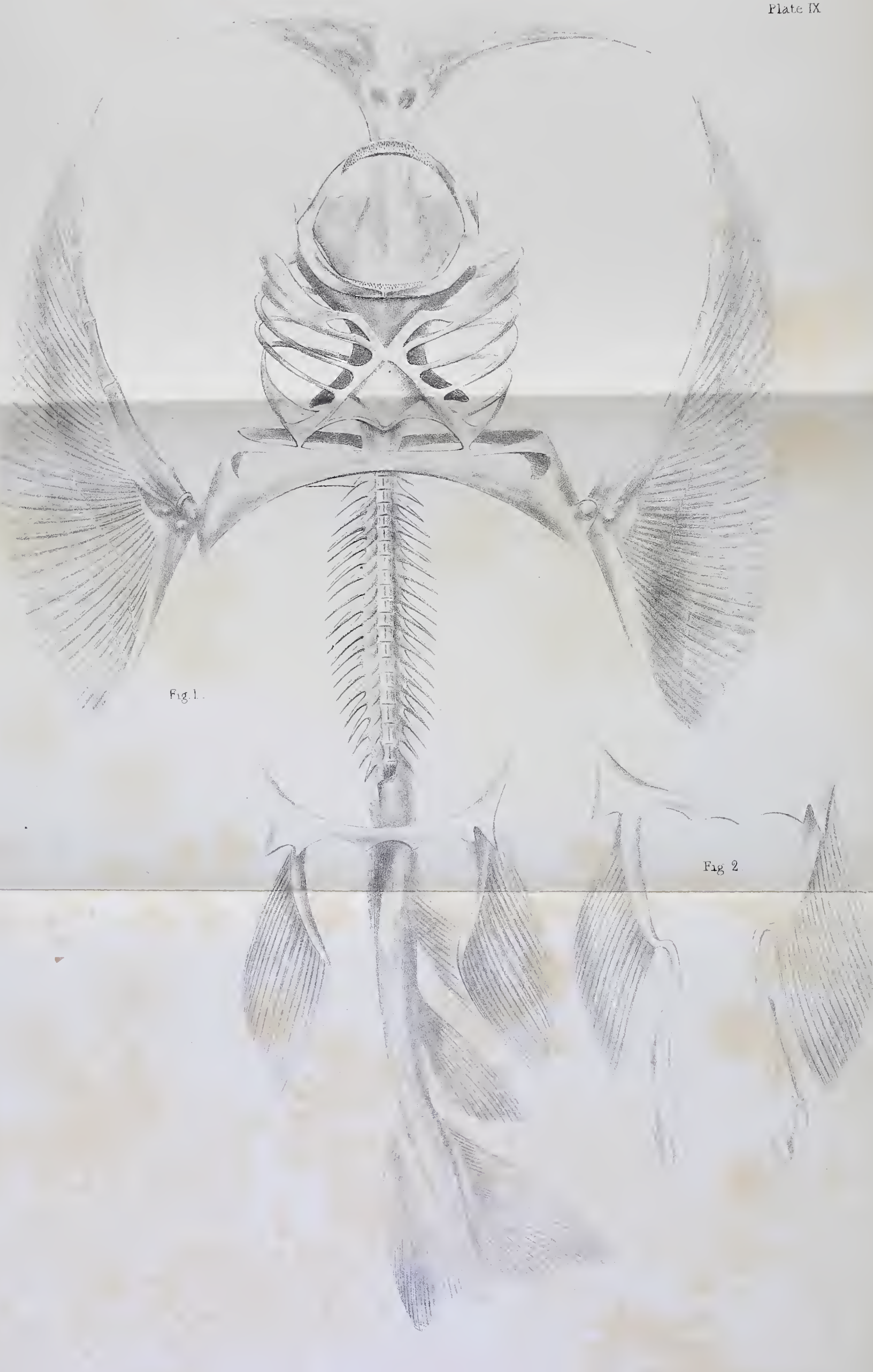

Fig. 1.

Fig. 2

Plate X.

Plate XII.

Fig. 2.

Fig 1.

Plate XIII